THURSDAY, JANUARY 26, 1888.

ODIUM MEDICUM.

NO one will deny the truth of the saying, "All men are mortal," but very few have any definite feeling that it applies to them personally so long as they are in the possession of health and strength. Almost everyone, however, has either suffered at a former time, is suffering now, or is afraid of suffering at some future time, from ailments of some sort; and therefore the treatment of disease has a personal interest for everyone. On this account the discussions which have been going on for about a month in the *Times* regarding homœopathy have attracted a good deal of attention; but it is difficult for lay readers to understand the merits of the discussion thoroughly unless they know something about the "pathies" generally. The fundamental idea of the "pathies" is that the body does not readily tolerate more than one diseased process at the same time, and therefore one morbid condition may be driven out by inducing another.

The nucleus of our present medicine may be said to consist of the accumulated experience in the observation and treatment of disease possessed by the priests of Cos, and recorded by Hippocrates, who is justly regarded as the father of medicine. His treatment was based upon empiricism, and was not governed by any absolute rule, for, although he stated that in general diseases are cured by their contraries, he also allowed that disease might sometimes be relieved by medicines which produced similar symptoms, and mentioned that under certain circumstances purgatives will bind the bowels, astringents will loosen them, and substances which cause cough and strangury will also cure them.

The principle that contraries are cured by contraries, *e.g.* that constipation is cured by purgatives, attained so much importance under Galen and his followers, that the other principle of like being cured by like was nearly lost sight of, and so the antipathic school had for a long time the preponderance. But the use of evacuants, which formed a large portion of the practice of Hippocrates and of medical practice down to the present day, could not always be brought under the head of antipathy, and so it came to be admitted that one abnormal condition in the body might be relieved by inducing another, which was neither of the same kind as itself, nor of an opposite kind, but was simply of a different nature, and this is the allopathic form of treatment. As an example of this we may take the fact that a pain in the head may be cured by a medicine which does not act on the head at all, but upon the bowels.

The antipathic and the allopathic systems of medicine were in vogue in the time of Hahnemann, and their imperfections were very evident to a man of his mental power and acuteness. He saw clearly that the enormous doses which were given in his time were often productive of great harm, and in experimenting with smaller doses he found that his results were better. He also found, what had been noted before by Hippocrates, that he obtained curative effects from small doses of remedies which in large doses produced symptoms similar to those of the disease. In the recognition of this fact Hahnemann agreed with Hippocrates; but, while the father of medicine, testing everything by experiment and relying simply on the result of experience, regarded the rule "*similia similibus curantur*" as only of partial application, Hahnemann converted it into a universal rule. He began at first by relying on experiment, and spoke of pure experience as the "only infallible oracle of medicine," but he afterwards quitted this sure ground, and committed himself unreservedly to a belief in his theoretical opinions, whether supported by facts or not, and said in regard to his doses that the maxim as to the very smallest being the best is "not to be refuted by any experience in the world.' The essence of his system of homœopathy consisted in the universal application of the rule regarding the similar action of the drug to that of the disease, and in the smallness of the dose.

Some modern homœopathists are inclined to regard the minute dose as not essential to homœopathy, but Hahnemann says: "The appropriation of the medicine to any given case of disease does not depend solely upon the circumstance of its being perfectly homœopathic, but also upon the minute quantity of the dose in which it is administered." The extent to which he carried the dilution of his medicines was extraordinary, and he imagined that the more they were diluted the more potent did they become. Thus he says in his "Materia Medica Pura" (Dr. Dudgeon's translation) that the curative power of aconite is marvellous when it is given "in the dose of a thousandth part of a drop of the decillionth development of power." But even this astoundingly minute dose was unnecessarily strong in some cases, in which he thought "a single momentary olfaction at a phial containing a globule the size of a mustard-seed, moistened with the decillionth potency of aconite, is quite sufficient." But it is difficult for those who have not studied the action of potent drugs like aconite to form any definite judgment regarding their effect in large and small doses; so that it may be worth while to give his views regarding vegetable charcoal, a substance about which everyone can form an opinion. Most people will be surprised to hear that Hahnemann gives no fewer than 720 symptoms as being caused by a few grains of vegetable charcoal diluted a million-fold with milk sugar. These symptoms are of the most varying nature, from aching of the corns to headache, palpitation, and rheumatism, with sometimes a peevish temper, and at other times an excessively cheerful one. The variety and severity of these symptoms clearly show that they were not due to the vegetable charcoal at all, but would have occurred whether the charcoal had been taken or not. But the most remarkable instance of a fallacy in Hahnemann's conclusions appears in his famous experiment on the action of cinchona bark in producing ague, which has been regarded by homœopathists as one of the most important proofs of the truth of the system. Hahnemann, at one time of his life, had suffered from ague, as we learn from Ameke's "History of Homœopathy," but he had probably been free from it for some time before he made his experiment with cinchona. It is well known that persons who have once suffered from ague are apt to have it return when their digestion is disturbed, or when they are subject to depressing influences. The dose of powdered cinchona bark which Hahnemann took was

very large, and similar doses have produced in other people vomiting and gastro-intestinal irritation. In Hahnemann it produced symptoms of ague, but instead of concluding that the cinchona had simply brought back an attack of his old enemy, by acting as an irritant to his stomach, he concluded that cinchona bark had a specific power to produce ague. Others who have tried the experiment, and who have not had ague before, have naturally failed.

Hahnemann's system was greatly ridiculed and opposed both during his life and since, and yet, in spite of its absurdities in regard to dose, it has a number of adherents. The reasons of this are perhaps not very hard to find. For instead of homœopathic medicines being disagreeable to the patient, as those of regular practitioners too often are, they are given in a form which is rather pleasant than otherwise, and Hahnemann's rules of diet and regimen were very different from those followed by regular practitioners of his time. While they were apt to consider that anything that seemed agreeable to the patient was dangerous and to be forbidden, Hahnemann, placing full reliance on the influence of his infinitesimal doses, allowed the desire of the patient for food and drink to be gratified within proper limits, and the temperature of the chamber as well as the quantity of the bed-clothes to be regulated according to the wishes of the patient. There can be no doubt that the attention given by Hahnemann and his followers to diet and regimen have been of great service, not only to the patients they have treated, but to the whole medical profession. It is obvious that such a system as Hahnemann's—gratifying the desires of the patient so far as it was judicious, giving remedies in such minute doses as could at all events do no harm, and at the same time encouraging the patient with the positive assurance that the infinitesimal doses were of the utmost potency to effect a cure—had a great advantage over the system of allopathy. This advantage was to a certain extent shared by antipathy, inasmuch as both it and homœopathy acted on a definite plan, and chose their drugs according to what they supposed to be fixed laws.

Although so far behind the other two in some respects, allopathy had this great advantage over them, that it depended simply on the results of experiment; and although it might be influenced, and was influenced at times, by prevailing fashions, its followers were still searching after truth, while the others falsely supposed they had already found it. With the development of pathology and a truer insight into the nature of disease, the term allopathy has fallen to a great extent into disuse, and most of what we might term the orthodox practitioners of the present day object to range themselves under any "pathy" whatever, but aim at the rational practice founded on the one hand upon the knowledge of the nature of disease, and on the other of the action of remedies. Where these are insufficient to guide them, they fall back simply upon empiricism; expecting, however, that before long wider knowledge may increase their power to cure their patients. Their power is no doubt very greatly on the increase; and we have only to look at the fact that within the last few years they have been able by the use of substances belonging to the aromatic series of chemical compounds to regulate the temperature of their patients, so that whereas formerly physicians were obliged to stand by idly while their patients died of high fever, they can now prevent the temperature from rising too high with almost perfect certainty, and thus save their patients' lives. Every day fresh contributions are being made both to the physician's knowledge of the nature of disease and his power to modify it or prevent it.

Yet still the regular physician is but a seeker after truth, and as yet no infallible rule by which to select his medicines is known to him. He cannot lay down with dogmatism that the medicine which he is about to administer is the only one or the very best one that can possibly be given, as a homœopath might do. He is therefore to a certain extent at a disadvantage as compared with the homœopath, especially in the treatment of those cases where the disease is not extremely severe, and where the effect upon the mind of the patient counts for as much or more than the action of the medicine itself. The want of a definite rule on the one hand affords an opportunity for the sneers of the homœopath at the regular practitioner, while at the same time he complains that the regular practitioner refuses to have any dealings with him. But there seems to be no other course open to the regular practitioner, for he considers that the homœopath must do one of two things: he either believes in homœopathy, or he does not. If he believes in homœopathy as founded by Hahnemann, and prescribes for his patients infinitesimal doses with a conviction that he is actually modifying the disease from which they suffer, the regular practitioner regards him as a fool; while he would apply a still stronger term to the man who does not believe in Hahnemann's system, and uses powerful drugs in large doses, but nevertheless professes to treat his patients homœopathically. It is as useless for a regular practitioner to treat a patient along with a believer in homœopathy as it is for a modern chemist to undertake a joint research with a believer in phlogiston; and therefore the regular practitioner refuses to meet him in consultation so long as he holds homœopathic doctrines. But if the homœopath gives up his belief in infinitesimal doses, and in the universal application of the rule "*similia similibus curantur*," he has given up the essentials of homœopathy, and has no more title to the name of homœopath than Hippocrates had. If he has given up the thing he should give up the name and join the ranks of orthodoxy, but if he still retains the name for the sake of gain he can hardly expect to be welcomed by the orthodox part of the medical profession. It is very unfortunate that the "*odium medicum*" should exist, but the homœopaths seem more to blame for it than the followers of rational medicine.

DARWINISM AND ETHICS.

The Ethical Import of Darwinism. By Jacob Gould Schurman, M.A., D.Sc., Sage Professor of Philosophy in Cornell University. (London: Williams and Norgate, 1888.)

Morality and Utility. By George Payne Best, B.A., M.B. (London: Trübner and Co., 1887.)

WE will consider these two little books together, as in some measure the latter, although earlier in publication, answers the former.

More than half of Prof. Schurman's essay (which altogether extends to but about 250 small octavo pages) is occupied with a preliminary discussion of Darwinian principles *per se*, or without special reference to ethics. Here his object is to argue in favour of "teleological variation" along "beneficial" or "predetermined" lines—supporting this argument in the usual way by denying that natural selection is a cause of organic change. Natural selection can only act on the materials supplied to it by variation: it does not itself create these materials, and therefore leaves where it was before, the question as to the *origin* of the fittest. This argument always appears to us transparently fallacious; but, as our object at present is to consider what Prof. Schurman has to say on "the ethical import of Darwinism," we will not occupy space by discussing the weaker half of his work. In ethics, however, he is strong; and, in our opinion, has produced one of the best defences of the intuitional side which has appeared since the publication of the "Descent of Man."

He begins by pointing out the distinction between the utilitarianism of Bentham and of Darwin—viz. the difference between "pleasure-giving and power-giving," or hedonism and life-serving. Next, he provisionally allows that the Darwinian theory furnishes a proximate or natural explanation of the "innateness, immutability, and universality of moral conceptions." He also allows that, at all events to a large extent, this theory is able to explain the authoritativeness of conscience. But, having thus frankly conceded all that the Darwinist has to demand, he turns upon him as follows:—

"Is it forgotten that, even if goodness be an end in itself—the sole end worth living for—it still remains true that honesty is the best policy, that honest acts are the most advantageous acts, and that they will accordingly be preserved through natural selection in the struggle for existence? All that natural selection requires is that something shall be useful; *what else it may be*, what other predicates it may have, natural selection knows not and seeks not. Be virtue a proximate or an ultimate end, natural selection tells us it will be preserved and perpetuated if it be useful; and it tells us no more. It is, accordingly, a gratuitous assumption which our exponents of evolutionary ethics make, when they decline to allow more than a merely relative value to morality."

The first thing to notice about this position is, that the Darwinian, *quâ* Darwinian, has nothing to do with it. All that the Darwinian, as such, undertakes to show is, that conscience and the moral sense, in all its protean forms, admit of being explained as proximately due to natural causes. Whether or not these natural causes are themselves the results of a final cause, intelligent and moral—this is a question which Darwinism leaves the ethical philosophers to wrangle about.

But now, suppose that a man is not only a Darwinian, but also an ethical philosopher, what is he to make of Prof. Schurman's conclusion that "it is a gratuitous assumption which our exponents of evolutionary ethics make, when they decline to allow more than a merely relative value to morality"? Surely such a man must feel that the burden of proof here lies with the intuitionists. It is they who affirm a supernatural quality of the moral sense, over and above the natural origin of it which (as agreed) the Darwinian has explained; therefore it is for *them* to show that *their* "assumption" of the *absolute* value of morality is other than "gratuitous." This burden Prof. Schurman seeks to discharge as follows:—

"In opposition to this mechanical theory of conscience, we hold that it is an ultimate function of the mind, and that in germ, as in full fruition, it must be regarded, not as an action, but as *an ideal of action*. . . . This view of the subject may be affiliated to Darwinism as readily as the other. For an abiding ideal of action is, to say the least, quite as beneficial as a chance action; and wherever there is an advantage, there natural selection may operate."

Now, without question, "an ideal of action is quite as beneficial as a chance action"; but is it not evident that the Professor is here proving too much? The more he can show that "an ideal of action" admits of being developed in the race by natural causes on account of its utility to the race, the more is he playing into the hands of his opponents, so long as they do not agree to assume with him that morality is of any absolute or ultra-human signification. But it is precisely this assumption which he is required to justify; and the above attempt to discharge his burden of proof, far from making "in opposition to the mechanical theory of conscience," is merely a re-statement of that theory, *plus* his original assumption.

Mr. Best is not nearly so matured a thinker upon ethical problems, and yet upon this important matter he displays a clearer vision than Prof. Schurman. He shows that the intuitive (or instinctive) side of conscience is everywhere an "ideal of action"; but he also shows that where it stands alone, or without reasoned perceptions of utility, it is everywhere an ideal impossible to realize. With considerable originality and success, he argues that the moral ideal, in all phases of its development, is essentially irrational, inasmuch as it could only realize itself completely in a population all the members of which "are equal, asexual, and immortal." He then goes on to ask:—

"Suppose such an idea should become actually operant, and endeavour to realize itself in thought, or in action, in this world of inequality, sexuality, birth and death, what kind of phenomenon might we expect to arise from the conflict between idea and fact? Might we not expect to find in those in whom the moral intuitions were best developed a constant protest against things as they are? Might we not expect to find a hankering after equality? Might we not expect to find some, in reaction against that inequality which, in the form of wealth, obtrudes itself before their eyes, take refuge in voluntary poverty; might we not expect others to endeavour by force or contrivance to bring about the reign of equality? Might we not expect the dim picture of an asexual world to make men revolt against sex and sexual relations, and cry up celibacy as the holiest condition possible?" &c.

Thus, then, the moral ideal is more or less out of joint with actual fact; and although it is easy enough to understand why such should be the case if it is but of relative significance—or of no further meaning than that which arises from its utility to the race—we cannot so well understand why such should be the case if it be of absolute significance. And, if we extend our view beyond the human race, we are met by a similar difficulty. Not only man, but the whole creation, groans in pain and travail—that is to say, the unmoral as well as the moral;

and, therefore, the creatures whose pain and travail cannot possibly serve any moral purpose. Yet the moral sense of man, in its most "intuitive" or least rational form, is outraged even by the practice of vivisection with a view to an ultimate amelioration of sentient life.

Our object in saying this much is to show that Prof. Schurman does not appear to have perceived the basal difficulty against which he has to contend. The question which he undertakes to answer is whether the moral sense is of absolute or only of relative significance. But this question he merely begs on behalf of the intuitionists. Of course, if it be thus assumed that the moral sense is of absolute significance, it is reasonable enough to show that the fact of its utility is not opposed to the assumption. But where the validity of this assumption is the matter in dispute, an intuitionist only plays into the hands of the utilitarian by arguing that in his view of morality "an ideal of action may be affiliated to Darwinism as readily as any other." Nevertheless, although we thus deem Prof. Schurman's essay a failure in its argument against the mechanical interpretation of conscience, it is otherwise an able contribution to the literature of ethics; and anyone who is already an intuitionist may properly accept the work as proving that there is nothing in Darwinism, *per se*, which can be logically regarded as inimical to his theory.

GEORGE J. ROMANES.

AN INDEX-CATALOGUE.

Index-Catalogue to the Library of the Surgeon-General's Office, United States Army. Vol. VIII., Legier—Medicine (Naval). (Washington, 1887.)

THE regularity with which the large annual volumes of this great work reach us is most reassuring, and now its completion in some six more years may be looked upon as practically certain, considering the vast resources of the United States, and the energy which its editors have shown. It still remains unique among printed catalogues in its immense lists of articles from every species of periodical literature, arranged under subject-headings, and drawn from more than 3400 Journals, Reviews, Transactions, &c. It has added to its list last year 165 new periodicals, and its tastes are sufficiently catholic to include such as the *Revue Philosophique*, which contains important matter bearing on the fundamentals of physiology and psychology, but hardly touching on any professional details.

The entries are carried up to the end of 1886; the volume has a few words of preface dated June 1887. When it is considered that the papers of Delhi, Madras, and Adelaide, for example, take some weeks to reach Washington, and that any of these may contain entries which should take a place in any part of this book of 1078 closely printed quarto pages, there seems to us certainly to have been no loss of time in publication. There are many entries, in this volume, of Chinese and Japanese books, magazine articles, and manuscripts, which the editors insert in English characters, and are kind enough to translate for us. Of the European languages also, Hungarian, Russian, and Polish are as a rule translated, much more freely than in the last volume; but Swedish and Danish rarely, and Portuguese, Dutch, Spanish, Italian, and Greek not at all. A very commendable practice has sprung up, though it is not found everywhere possible, of putting the date of birth after a living author's name. Thus we read Lussana (1820—), Luys (1828—), &c. It would be very convenient if this could be further extended, though of course the difficulties in the way are obvious. There are some very large collections of entries under such words as Liver (70 pp.), Lungs (30 pp.), Lithotrity and Lithotomy (40 pp.), and the extent of the bibliography is well illustrated when we find 213 books and 646 articles entered under such a simple heading as Measles. By far the largest aggregation, and one as yet unfinished in this volume, is under Medicine, which in the present volume occupies 288 pages. It is a heading under which the subdivisions have been difficult to arrange; but the large bulk of matter has, on the whole, been well distributed. Under such a subdivision as Medicine (Anecdotes, Curiosities, &c.), we naturally find strange companions, such as "Uriel to his Compeers; adapted by Ithuriel"; "The Doctor, by the Author of 'Betsy Lee'"; "Sniggers (J.), Gnihtontuobaodahcum," the last a *Spiegelschrift* in print. Under Medicine (Systems, Theories, and Practice), we find a large group of the elder writers who are chiefly of historical interest, extending from "Averrhoes: Incipit Liber de medecina Averoys qui dicitur Coliget, &c., imp. folio, Venetiis, 1482 (Gothic letter), to the writers of the last generation, such as Dr. C. J. B. Williams (1842), and, curiously enough, containing only one small volume among the modern hand-books, "Elements of Practical Medicine, by A. H. Carter, 1881," which might have come more appropriately among Medicine (Manuals) or Medicine (Practice of), along with the mass of modern text-books. Groups are chronicled under Medicine (Magical, Mystic, Spagyric) of some 300 books, and of some 250 under Medicine (Chronothermal, including the Thompsonian system), which serve to remind us of the chequered history and varied principles of the healing art. To the accuracy of this vast body of references, amounting to more than 40,000 in all, it is Time that will bear the best testimony, as it has borne to those of the earlier volumes. A first testing on such detail as is practicable shows the figures right, and the text sometimes—as, for instance, in the case of M. Luys—more accurate than that of the author's own publisher in his advertisement columns. It is a mistake, we must allow, but we trust a very pardonable one, to have spelt the name of a distinguished living physician as "S. Wilkes"; and it is a pity that, under the record of Hospital Reports (London), we should find mention only of those of the Hospitals of St. Thomas and St. George. But these are trifles; when we close the heavy volume we cannot help feeling a hearty admiration of so much hard and careful work well spent, not on the aggrandizement of any individual fame, but on the steady and strenuous advancement of learning.

A. T. MYERS.

OUR BOOK SHELF.

A Vertebrate Fauna of Sutherland, Caithness, and West Cromarty. By J. A. Harvie-Brown and T. E. Buckley. (Edinburgh: David Douglas, 1887.)

THIS is a good type of all that a hand-book on local natural history ought to be from a naturalist's point of view. While it appears to be as exhaustive as any two

workers can make it of the fauna of which it treats, its honest tale is not only plainly but also briefly told. In other words, we are spared those poor attempts at poetical prose and all the allied sins which seem so easily to beset the field naturalist. This is another way of saying that the work has been undertaken and executed in a purely scientific spirit. After a few introductory chapters on the geography, topography, physical aspects, &c., of the area, the authors proceed to give a systematic catalogue of the entire vertebrate fauna, beginning with the mammals and ending with the fish. In this catalogue everything relating to distribution, habits, &c., which can possibly be of any interest is likewise set forth in terse phraseology. The whole catalogue covers between 200 and 300 octavo pages, and is everywhere indicative of painstaking labour. Several well-executed plates embellish the volume, which throughout displays good taste as well as sound judgment. We are, therefore, particularly glad to read in their preface that the writers intend this to constitute "the first volume of a series, which, unlike most local faunas, lays aside to a great extent political boundaries, and is marked out by others, much more natural, such as watersheds." We trust that this first volume will meet with the recognition which it deserves; and in any case congratulate the writers on having so successfully accomplished so extensive and valuable a piece of work. G. J. R.

Gospel Ethnology. By S. R. Pattison. (London: Religious Tract Society.)

THE author observes that the many-sided investigations of ethnologists do not seem to have included a study of the way in which Christianity has been received by different races. The problem suggested is undoubtedly an interesting one, but the present volume does not do much to solve it, being mainly made up of a series of anecdotes which go to show that the Gospel, in the form in which it is set forth by Protestant missionaries of the Evangelical school, has found a response in the hearts of individuals of almost every known race. It is reasonable to infer from this that the particular type of Christianity to which Mr. Pattison confines his attention contains motives that appeal to men in almost every stage of social development and culture. But so far as one can judge from the anecdotes, which are not chosen with a view to facilitating scientific analysis, it seems probable that in every case the really effective element in the missionary teaching lay in the Gospel story itself, not in the dogmatic construction built on it by missionaries of a particular school. At any rate, it is plain that no discussion of the problem which Mr. Pattison deals with can claim to be of scientific value so long as it deals only with the reception given to one form of Christian teaching. Nor is it enough to know that *individuals* of almost every race are capable of becoming sincere Christians of a particular school: the ethnologist, from his point of view, is much less concerned with individuals than with masses. A serious inquiry into the fitness of Christianity to become the religion of *societies* that have not come under the influence of the civilization of the Roman Empire would be extremely useful, but such an inquiry cannot be made to any purpose if one starts by identifying Christianity with one of its local and particular types.

There is not really any ethnology in the book before us. There is, indeed, a chapter which professes to give a survey of the races of mankind, but it is so badly done that the book, which is really, as has been said, a collection of anecdotes, would have been better without it. There are some good woodcuts of people of different races.

The British Journal, Photographic Almanac, and Photographer's Daily Companion for 1888. Edited by J. Traill Taylor. (London: H. Greenwood and Co., 1888.)

To all those who are engaged in the art of photography either as amateurs or as professionals this work will be extremely useful. Besides all the ordinary information, such as that of developing, toning, &c., there are articles of the most practical and theoretical nature—written by such men as Captain Abney, F.R.S., Rev. S. J. Perry, F.R.S., &c.—on subjects which are most interesting, and of great service to those who have attained the higher branches of the art.

All the various tables and formulæ are here added, together with a list of all the Photographic Societies at home and abroad.

Two pictorial illustrations are given, one being of the famous yacht *Thistle*, printed by Messrs. Morgan and Kidd on their argentic bromide paper (360 being able to be printed from one negative in an hour) from a negative by Mr. A. H. Clark; the other being a callotype by Messrs. Waterlow and Sons from a negative by Mr. T. B. B. Wellington on a Pall Mall plate, entitled "You naughty boy."

For the sake of young photographers the Editor has written twelve chapters to present in simple language a few lessons in the practice of the art.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"A Conspiracy of Silence."

WHEN I read Prof. Judd's letter in your issue of last week, I felt as if one of the Eocene volcanoes of the Isle of Mull, which he has described so well, had broken out afresh and covered a great extent of country with erupted matter, decidedly, by a wonderful phenomenon, of the "acid series."

I have a very short reply to give:—

First, it is not the fact that I have made any attack on geologists. The fathers of British geology were among my dearest and most intimate friends, and I have the highest respect for many of the (comparative) specialists among whom, by the division of labour, the science is now divided. Among the most eminent of these I have always reckoned Prof. Judd himself.

Secondly, it is not the fact that I have accused anyone of conscious indifference to truth. I attacked the undue influence of authority in science, and in doing so I used the well-known formula "conspiracy of silence," which, on the face of it, is a metaphorical and rhetorical expression, but which has been used in his latest writings by Prof. Huxley precisely in the same sense, and has been applied by him to the most distinguished scientific body in the world—the French Institute.

Thirdly, it is not the fact that I have challenged discussion on my September article upon "Coral Reefs." I have challenged discussion upon the subject, and on the question of Darwin's theory—of which my paper was a mere popular abstract, and nothing more.

Recent discoveries by the staff of the *Challenger*—the observations of Prof. Semper—the papers of Mr. John Murray, and of Mr. L. Agassiz—and lastly, the recent admirable observations of Dr. Guppy, have, in their combination, afforded ample ground and materials for a review of the whole question; and I have a distinct opinion, which I repeat, that the influence of Darwinian authority and prejudice is one of the causes which has retarded, and is now retarding, any acknowledged solution of the question.

I have heard with extreme regret that Dr. Guppy, the most recent witness to facts irreconcilable with Darwin's theory, has felt compelled to resign his position as member of the London Geological Society—for what reason I do not fully know, but for some reason connected with his views on this subject.

Inverary. ARGYLL.

On some Unapparent Contradictions at the Foundations of Knowledge.

An argument parallel to that by which Mr. Tolver Preston proposes to prove that Space is nothing will prove with equal cogency that Time is nothing. But if Space is nothing and Time is nothing, then he has the choice of two alternatives, neither of which will he find particularly acceptable. If Space and Time are both nothings, they are identical. If Space and Time are not identical, then they are two nothings which differ. What is the difference between two nothings?

I would suggest that Mr. Preston should read Mr. Herbert Spencer's views on "The Relativity of Knowledge," contained in Chapter IV. of "First Principles." On his carefully thinking this out, and understanding it, I am willing to hope that the title I have adopted for this letter may appear to him appropriate to the subject-matter which he has brought under the consideration of your readers. F. Howard Collins.

Churchfield, Edgbaston.

Extraordinary Fog in January 1888, at Shirenewton Hall, Chepstow.

The recent fog has been so remarkable that it seems desirable to record its principal features. From the 7th to the 14th the air was completely saturated with moisture. The most notable feature was that of cold air passing over a warm ground, for from the 11th to the 15th the greatest cold on the grass did not descend to that read at 4 feet. Such a condition of the air as this has not been noticed since I commenced observations in 1838.

The following readings of the thermometers will illustrate this:—

Date.	Temp. 4ft. 10 a.m.	D.ff. bet. wet and dry.	Min. 4 ft.	Min. grass.	Diff.
Jan.	°	°	°	°	°
7	43·3	0·0	40·0	34·5	−5·5
8	45·6	0·0	42·1	37·2	−4·9
9	41·2	0·0	37·0	30·3	−6·7
10	34·3	0·0	32·8	28·1	−4·7
11	36·7	0·0	33·3	37·3	+4·0
12	29·8	0·0	29·3	30·5	+1·2
13	28·3	0·0	26·7	28·3	+1·6
14	32·0	0·0	25·0	29·5	+4·5
15	33·9	1·0	28·0	30·0	+2·0
16	30·0	0·5	27·0	27·0	0·0
17	30·5	1·0	29·7	29·7	0·0
18	28·6	0·0	27·0	27·0	0·0
19	31·7	0·4	26·4	24·8	−1·6

Throughout the 12th after 9 a.m. the temperature on the grass was above 32°, whilst it was a frost from the height of 1 foot upwards; at 10 a.m. the temperature on grass was 32°·8, at 4 feet 29°·8, and at 10 feet 28°·6.

The fog lasted from the evening of the 6th till 3 p.m. of the 14th. On the 7th the clouds moved rapidly in W. current, and on the 8th they moved rapidly in S.W. current; on the 9th nearly calm and cloudless overhead; from the 10th to 14th overcast (except from 11 a.m. on the 12th till 12.40 p.m.). The chief direction of the wind was: 8th S.S.W., 9th S.S.E., 10th W.S.W., 11th and 12th calm, and from 13th to 18th between N. and N.E., and on the 19th E.S.E.

The fog was wet and yielded much moisture, viz.:—

7th ·079, 8th ·008, 9th ·015, 10th ·017, 11th ·031, 12th ·013, 13th ·020, 14th ·020, 15th ·023.

The barometer was very high, and almost stationary, reaching a maximum on the 9th at 10h. 30m. a.m., viz. 30·75 inches corrected and reduced to the sea-level.

On the 11th the fog cloud moved in a south current till 3 p.m., when it became north, and continued so throughout the 12th. On this day on the side facing the fog current every leaf and twig had a horizontal deposit of ice, increasing in length from half an inch at 4 feet above the ground to fully an inch at 10 feet; the outside edge of this ice being as thin as the fine edge of a knife; and the whole upper surface of all laurel and other large leaves that were horizontal had a coating of ice, so thin (although it could be detached without breaking) as almost to resemble gold-leaf, on which were transparent impressions of every irregularity, however minute. On the side of trees opposite to this current, instead of rime there were nearly pear-shaped transparent drops of frozen water, of various sizes, mostly as large as one-eighth of an inch in diameter; they were situated *not quite* at the point of every leaf; no leaf was without a frozen drop, and this had an extraordinary appearance, more especially amongst the crowded leaves of such plants as *Pinus insignis*, *Abies Webbiana*, &c. On the opposite side of these fir-trees the appearance was equally singular, as each leaf looked like a knife-blade of one-sixth of an inch in width, with a square apex. The ground-temperature being above 32°, the vivid green of the grass was a great contrast to the ice on the trees. E. J. Lowe.

"The Art of Computation for the Purposes of Science."

In a paper with the above title, in Nature, vol. xxxvii. p. 237, Mr. Sydney Lupton refers to some of our work as affording a good example of "the natural tendency of the human mind . . . . to exalt the accuracy of one's own experiments."

The experimental work referred to was a determination by the dynamical method of the vapour pressures of liquid benzene. A curve was drawn to represent these relations; three points were chosen, and the constants for the formula $\log p = a + ba^t$ were calculated. Mr. Lupton finds fault with the number of decimal places given for these constants, and makes three statements which are intended to put the experimental work in as unfavourable a light as possible so as to heighten the contrast with the extreme accuracy of the calculations. Mr. Lupton says: "Nine places of decimals are given with apparent confidence, when (1) only three of the whole number of experiments were made even in duplicate." We do not quite understand this statement, for on reference to the original paper (*Phil. Mag.*, Jan. 1887) it will be seen that the last six experiments in Series I. overlap the first six in Series II., while the last seven of Series II. are within the same limits of temperature as the first four of Series III. The second statement is that "the last pressure, 755, was obtained not by experiment at all, but by extrapolation from a freehand curve, the highest experiment being 79°·6 and 743·1 mm." We would point out that the experiment referred to is not the highest, for on the preceding page in our paper the boiling-point 79°·9 at 753·4 mm. is given. Again, the curve was not drawn by freehand, but by means of engineers' curves, which give very much more accurate results. It is quite true that the last pressure was obtained by extrapolation, but an extrapolation of 0°·1, or even of 0°·4 does not seem very excessive with a range of 80°. Mr. Lupton states, thirdly, that "a difference of $\frac{1}{8}$° at low temperatures produced no change in pressure which was appreciable by the apparatus used." But, as a matter of fact, at 0° a difference of 0°·1 corresponds to a difference of pressure of 0·15 mm., which is quite appreciable on our gauge. Perhaps, however, Mr. Lupton refers to the experiments at 36·15 mm., in which at the same pressure two *different* thermometers registered temperatures which differed by $\frac{1}{8}$°.

Mr. Lupton lastly gives much simpler constants, calculated from our data, and compares the pressure at 60°, calculated from them and from our constants, with the pressure given by Regnault. It happens that the number obtained with the simpler constants exhibits greater concordance with Regnault's value. Now while we would agree with Mr. Lupton in classing Regnault (as far at least as some of his work is concerned) with the select few who are entitled to an extra number of decimal places, yet we would point out that Regnault did not always succeed in obtaining perfectly pure substances to work with, and some of his results are rendered almost valueless on that account. In this case, for instance, the melting-point of Regnault's sample of benzene was 4°·44, whereas after the most careful purification we find that it melts at 5°·58, and the value obtained by Fischer (*Wiedemann's Annalen*, xxviii. 400) is almost exactly the same as ours. Again, Regnault failed to observe the existence of a difference in the vapour pressure of solid and liquid benzene (and other substances) at the same temperature, while this difference has been measured by Fischer by the statical and by ourselves by the dynamical method.

We are quite willing to admit that our decimal points are carried further than is necessary for the calculation of the vapour pressures, but we have frequently had occasion to calculate the values of $\frac{dp}{dt}$ for various substances, and we have found that in order to obtain regular values a large number of decimal places are required; if a smaller number are employed the

values of $\frac{dp}{dt}$ themselves require smoothing, which involves additional labour.

But if—since vapour pressures only are given in our paper—we have gone to one extreme, we think that Mr. Lupton has gone to the other, for at 79°·9 the pressure calculated from his constants differs by 3·8 mm. (= 0°·16) from that calculated from ours, and by 3·1 mm. (= 0°·13) from our observed pressure, and this difference is certainly too great.

It might also be supposed from Mr. Lupton's constants that the value of b in the formula $\log p = a + b\alpha^t$ could generally be expressed by a very simple number such as the one he gives (− 3·3), but this is not so. It happens that our constant differs only very slightly from the number − 3·3; it is − 3·30062, and by striking off the two last figures in this constant and making a corresponding slight alteration in the value of a, a much greater simplification is possible than would usually be the case.

Mr. Lupton gives five decimal places for log α, and we are unable to appreciate the advantage of using a table of four-figure logarithms where five places are required.

While recognizing the advantage of methods of computation, may we suggest, in conclusion, that, as a rule, only experimentalists are capable of judging the limits of accuracy of experiment, and that they may be trusted to save themselves trouble where trouble may be saved without sacrificing accuracy?

W. RAMSAY.
SYDNEY YOUNG.

"The Mammoth and the Flood."

THE question raised in my previous letter is too important and is being too widely discussed to allow me to let it go by default, and as it has a certain freshness I cannot help thinking that it will prove interesting to many of your readers.

Your critic disposes of Sir Andrew Ramsey in a very unceremonious fashion. To describe the head of the Geological Survey, and the former President of the British Association and the Geological Society, as *an irrational uniformitarian* is to get rid of my attack in a very simple way. Surely some of his scholars or some of his subordinates will have a word to say for their late chief, and, if they cannot maintain his position, will offer some alternative. To the great mass of scientific men who are not geologists, teaching from such a source is naturally accepted as authoritative.

To pass on, however. Your critic speaks of my invoking a series of catastrophes to explain the difficulties surrounding the extinction of the mammoth. This is most inexplicable to me, and points to his not having read my book at all, which was neither fair to you nor me. My book is a perpetual protest against such a series of catastrophes, and an argument in favour of one catastrophe only. May I quote one statement among others?

"If we are to summon some normal cause not now operating for these facts, it certainly seems more reasonable that, with effects so completely alike over such a wide area we should summon *one cause*, and not *several*, and attribute the aberrant conditions showing so much uniformity to some uniform impulse. Here, again, the burden of proof is upon those who deny this view, and treat the remains not as the result of some widespread catastrophe, but as evidence of as many catastrophes as there are skeletons.

"It would be as unreasonable to invoke a separate storm and a separate date for the death of each one of the myriads of razor-bills and guillemots that strewed the western coasts of Britain on a fatal occasion a few years ago, and whose remains were all fresh and in the same condition, as to do the same for the myriads of fresh skeletons of mammoths, rhinoceroses, bisons, &c., in Siberia or in Europe. These debris of a former world have every sign that they formed parts of a contemporaneous fauna destroyed at one time, and are not the wreckage of centuries of deaths."

I now come to what is more important; namely, the theory which your critic resuscitates, after it has been given up by all the Russian inquirers, save one, for many decades—namely, the notion that the mammoths have been floated by the rivers from some undefined land and buried by river action, where they are now found.

Dr. Bunge, who has recently returned from a protracted residence on the Lower Lena, and has described his researches in detail before the St. Petersburg Academy, tells us expressly that mammoth remains are found very seldom indeed in the delta of the Lena, and very seldom also near that river. It is in the higher land separating the great rivers that the remains abound, and especially, as Wrangell and others showed long ago, and as Bunge has recently confirmed, in the mounds and low hills of the tundra. When found near rivers, it is near the short rivers, like those of North-Eastern Siberia, or near the head streams of the Lena, the Yenissei, &c., which could not float such carcasses.

In the next place, Northern Siberia is not a country of mountains and small valleys, but a vast, continuous, nearly level waste covered with moss, called a tundra, diversified by mounds and rounded hillocks, and threaded here and there by rivers running in deep channels—rivers which are frozen fast for a large part of the year.

When the late spring comes, and the ice in the upper reaches melts, while that lower down is still locked fast, there is no doubt a considerable flood in the estuarine parts of the Obi and other rivers, but this is temporary and transient, and it only covers the low lands where mammoth remains are most infrequent. It never covers and cannot cover the higher land. There is not supply of water to do it. To cover the higher points where the mammoth and other remains abound would require such a supply as would put the whole northern part of the continent under water, and thus destroy all animal life there every spring flood. Even if we could postulate river floods of this kind as I have shown, quoting a most experienced geologist, Schmidt, the Siberian rivers deposit no warp that could cover in the mammoths as they are found covered in, by deep beds of clay and gravel, not when lying on their sides only, but when standing upright, as they have several times been found. They must have been covered in by more than two yards of deposit also in a single year in all parts of Siberia, since the ground melts to that depth in the summer, which melting would destroy their soft parts. Appeals to river-floods therefore involve appeals to transcendental causes which are obsolete in other sciences than geology.

Lastly, why is this river portage invoked at all? We have not merely the mammoth carcasses to account for, but the trees found with these great beasts *still rooted*, and the land and freshwater shells showing a different climate when he lived.

Where are we to bring these debris of a former life from? We cannot go outside of Siberia; for the mammoth, so far as we know, has never been found in Asia outside that province. We cannot bring the mammoths found in Kamchatka, and the peninsula of the Chukchi, and the Liachov Islands (which are 150 miles from the mainland), from Central Siberia. Again the remains are very infrequent there compared with their abundance further north, while the mammoths from the north and south of Siberia can be discriminated. There is no sign of rolling on the bones, and the epiphyses are still attached. Evidence of every kind converges therefore upon the conclusion that the mammoths lived and died where their remains are found, and the problem that has to be faced is, how they were exterminated simultaneously from the Obi to Bering Straits, of all ages and sizes, and mixed with various incongruous beasts; how they were buried in the hillocks and high ground under vast, undisturbed, and continuous beds of gravel and clay; and how, lastly, their flesh was subsequently preserved. If all this can be explained without some appeal to the forces I have invoked, then *one factor out of many* in my argument can be answered. If not, it is no use going to Wonderland for hypotheses which only arouse ridicule among students of those sciences which claim induction for their basis. I am most anxious for an answer.

HENRY H. HOWORTH.

Bentcliffe, Eccles.

Is Hail so formed?

I NOTICED here yesterday a curious phenomenon—one that has not before come under my observation.

I was standing under a pine-tree that was laden with moisture from the foggy atmosphere; drops were falling to the ground from the branches, but what struck me was the fact that although most of the drops reached the ground in a liquid state, some of them were converted in their descent into *pellets of ice*.

It was very cold, but I had no reliable means of ascertaining the temperature at the time; it could not, however, have been far off freezing-point.

I was quite unable at first to account for the fact that some drops were frozen while others were not; it occurred to me later, however, that the drops which reached the ground as pellets of ice had been derived from the *topmost* branches, while those remaining uncongealed had fallen from the *lower* ones.

I based my conclusion on the assumption that the drops from the top of the tree in falling a greater distance, and in travelling more rapidly, than those beneath them, consequently suffered a greater loss of heat by more rapid evaporation, and hence were converted into ice before reaching the ground; but it seems to me nevertheless a most remarkable thing that such a result should depend upon so small a difference in altitude (10 ft. at the most), and the atmospheric conditions favourable for the production of such a phenomenon must have been so unusual as to make its recurrence very unlikely.

I have heard of a railway train becoming coated with ice after travelling through an atmosphere above freezing-point and laden with mist, but we can easily grasp the phenomenon when occurring on so large a scale.

CECIL CARUS-WILSON.

Bournemouth, January 14.

"British and Irish Salmonidæ."

ALTHOUGH calling in question statements made by reviewers is generally a thankless task, still, when an author believes himself to have been misquoted as well as erroneously corrected, a deviation from the usual course may sometimes be excusable. Acting under such an impression, and feeling sure that the Editor of NATURE, and the reviewer of my "British and Irish Salmonidæ," would be equally unwilling to promulgate errors to the public, I must ask for a small space with reference to the review of my work which appeared in your last issue.

Purporting to quote a sentence of mine (p. 31) as an example of my "originality in sentence construction," the reviewer has rendered it misleading by omitting five words which I have here re-inserted in italics and within brackets. Alluding to the water containing the recently expressed eggs and milt, he makes me say as follows:—"This is gently stirred with the hand (*and then allowed to stand*) until the eggs harden, or 'frees' as it is termed, being a period from one-quarter to three-quarters of an hour," &c. If newly expressed eggs and milt were thus stirred up from fifteen to forty-five minutes they could not "set," and would therefore have no occasion to "free," as the Americans have termed it, but such misplaced energy in the operator (which I never proposed) would assuredly destroy their vitality.

The reviewer says, "the description in the text of the mode of packing eggs which has been perfected at Howietoun seems to be erroneous, . . . while in a quotation in a footnote the correct account is given—namely, that the ova lie in direct contact with the damp moss, and are covered by another layer of the same, the muslin being only used in order that the layer of moss may be lifted and moved." The reviewer has here confused the text, or general principles as laid down, with the note (p. 42) of the mode pursued at Howietoun, which he asserts to be "the correct account"; but had he read the quotation to the end he would have seen that, besides the plan adopted at Howietoun for packing eggs going *long distances* when no muslin is used, a second mode is employed for those going *lesser journeys*, and was described as follows: "For shorter journeys eggs are thrown off the frames on to swans' down, which takes little more than half the time, and greatly facilitates the unpacking at the end of the journey."

The reviewer observes that "no reference is given to any work where the correct description of *S. namaycush* as a char can be found." If this remark is seriously made under the impression that the fish is not a char or a *Salvelinus*, I would refer among others to *Salvelinus namaycush*, Jordan, Bull. 16, U.S. Mus. 1883, p. 317; Bean, "Fish Com. Report," 1884, p. 1042; Garman on the "American Salmon and Trout," Boston, 1885, p. 5; to Brown Goode in his "Game Fishes of the United States," and his more recently published account in the "Fisheries and Fishery Industries of the United States," 1884, p. 485, &c. In this last work he observed of the *namaycush* that "the Lake trout is in fact a member of the same group of the salmon family with the chars," while I referred to his statements at p. 249.

FRANCIS DAY.

Cheltenham, January 14.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

IN June 1884 we called the attention of those who are interested in science and in the science-teaching in our public schools to some new regulations for admission to Sandhurst which had lately been announced, and to the efforts that had been made by the President of the Royal Society, and others, to induce the authorities at the War Office to reconsider their scheme, which appeared likely to seriously handicap those public schools in which real attention to science is given in the regular school work, and to be unjust to young men of scientific ability.

Whilst we wrote, those regulations were undergoing revision, and they were subsequently replaced by others in which certain improvements were made, but in which the mark value of science was still so low as to be likely to do some harm. In a second notice of the subject in August of the same year, whilst admitting that improvements had been effected, we expressed our opinion that even in their new form the regulations would tend to check freedom and progress in education, and act unfavourably on the work of those public schools which have aimed at widening the basis of education by introducing the study of physical science into the regular school work of all, or nearly all, their pupils.

We regret to add that this view has proved to be, to a considerable degree, correct. We hear, for example, that almost directly after the issue of the amended regulations all work in science was omitted from the instruction given to boys at once upon their deciding to become candidates for Sandhurst, in at least one large school. In the interests of the subsequent career of the boys this was, and is still, considered to be necessary. And we notice that at the last four examinations only about 2 per cent. of successful Sandhurst candidates offered a knowledge of some branch of physical science, ("experimental science" in the regulations), where formerly the very moderate but much larger proportion of 8 per cent. did so. In the case of physical geography and geology the corresponding proportions are 19 per cent. during the four years that preceded the date of our article, and about 8 per cent. during the last two years or thereabouts.

No doubt the candidates for Sandhurst are not, as a rule, drawn from the classes to whom the study of science is particularly attractive, and it is not impossible that to some extent the present regulations for admission to Sandhurst may have had the effect of inducing the scientific boys to enter more fully for the scientific branches of the army, to which admission is gained through Woolwich.

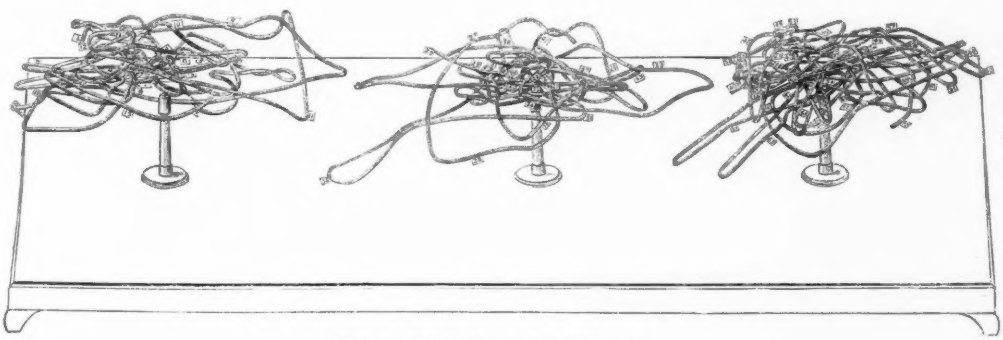
In the examinations for Woolwich, science has hitherto met with more liberal treatment than at Sandhurst, and has been taken up by a fair, but not excessive, proportion of the successful candidates, which has lately tended to increase in the case of chemistry and physics. It is therefore with the greatest regret that we learn that new regulations for admission to Woolwich, which are to come into effect in November, have been issued, which will be likely to seriously further discourage the teaching of physical science. These regulations correspond pretty closely to those for Sandhurst, which we have previously discussed; it will be sufficient, therefore, to say that compulsory mathematics, optional mathematics, Latin, French, and German, form Class I., have each of them an allotment of 3000 marks; that Greek, English history, chemistry, physics, physical geography, and geology, form a second class, to each of which 2000 marks are allotted; and that candidates may take all the subjects of Group I.,[1] or may substitute one subject from Group II. in place of one of those in Group I.

[1] They may also take any or all of Group III., English composition, freehand and geometrical drawing, to each of which 500 marks are allotted.

Hitherto the mark values of all subjects, except mathematics, have been equal, and free choice of subjects has been permitted to candidates. This has been fair to young men of different orders of ability; it must have secured officers of varied powers, and has satisfied the schools by leaving them free to do for each boy that which was best for him. In one respect the new scheme is better than the old—viz. in the grouping of the physical science subjects. But with such a bribe as will now be offered for Latin and modern languages, we cannot think that it will usually be worth while even for boys of more than average scientific capacity to adopt the study of science if they desire to enter Woolwich. It is evident that, other things being equal, those who do so will come out lower in the list of those who succeed, and be more likely to find themselves amongst those who have failed, than will be the case with such as are of equal ability in the study of languages. We do not believe that it is the intention of the War Office authorities thus to partly bar the way into the scientific branches of the service against young men of more than average promise in the experimental sciences that will afterwards form so important a part of their work in the Royal Military Academy; and we trust that leaders in science, and the representatives of those schools which are doing their best for their scientific boys, as well as for their unscientific boys, will not fail to unite in calling attention to the probable results of the final adoption of the present scheme. The reception that such representations met with in 1884, and the position accorded to physical science in the course of study for the cadets after having entered Woolwich, cause us to feel sure that such representations will not be without effect. But they must not be too long delayed.

A MODEL OF AN EARTHQUAKE.

IN the latest part of the Journal of the Science College of the University of Tokio, Prof. Sekiya describes a very curious and remarkable model he has made to exhibit the manner in which a point on the earth's surface moves during an earthquake. Readers who have followed the recent progress of seismometry in Japan are aware that the motion which is recorded at an earthquake observatory is a prolonged series of twists and wriggles of the most complicated kind, so that the path pursued by a point on the surface of the soil has been aptly compared to the form taken by a long hank of string when loosely ravelled together and thrown down in a confused heap. Prof. Sekiya has taken advantage of a very complete earthquake record obtained by him with a set of Prof.



Professor Sekiya's Model of an Earthquake.

Ewing's seismographs to follow out this path step by step, and to represent it, in a permanent form, by means of stiff copper wire. The earthquake he has modelled in this way took place on January 15, 1887, and was unusually severe, for Japan. It has been already described in NATURE (vol. xxxvi. p. 107), and we have given there a copy of the seismographic record by help of which the model has been constructed. The seismogram shows the vertical displacement and two rectangular components of the horizontal displacement, instant by instant, throughout the disturbance.

It was only necessary to go through the laborious task of compounding the three displacements in order to find the actual path. This, Prof. Sekiya has done for the first seventy-two seconds of the earthquake—a period which embraces all the most interesting features, although large movements in a horizontal plane continued for a minute more, and small movements for a still longer time.

After the seventy-second second, however, the vertical component of motion had virtually disappeared, so that the later part of the disturbance might be represented by a curve drawn on a horizontal plane. To avoid confusion, the model (a sketch of which is given above) is constructed in three parts: the first and second parts each refer to twenty seconds, the third to thirty-two seconds. The parts are mounted together on a lacquered stand 3 feet long, genuinely Japanese as to its legs, as the sketch will show. The model represents the absolute motion of the ground magnified fifty times. Little metal labels are attached to the wire to mark successive seconds of time, from 0, where the shock begins, to 72, where the model ends.

Prof. Sekiya is to be congratulated on his patience and skill. The model will serve to show at a glance the real character and enormous complexity of earthquake motion; it may also serve to open the eyes of seismologists of the older school to the perfection to which earthquake measurement has now been brought. We learn by a Japanese advertisement that a native firm (Seirensha and Co., Tokio) has undertaken to sell copies of Prof. Sekiya's model, lacquered stand and all, at a price so low that it should induce many private persons, not to speak of curators of museums and others officially interested in scientific novelties, to possess themselves of this pretty and instructive Japanese "curio."

ANTON DE BARY.

ON January 19, after a painful illness, died Anton De Bary, for many years the Professor of Botany in Strassburg. He had been suffering for some time since his visit to this country in September, and had undergone an operation which entailed the removal of parts of the face, but he did not recover.

De Bary's influence on the progress of biology has been enormous, and in attempting to form an estimate of the value of his contributions to science, we must try to picture the state of botany in 1850 or thereabouts, when his labours began.

Little was known of the Thallophytes beyond the apparently endless species-making, which was coming into shape, however, under the discriminating hands of Agardh, Harvey, and Kützing; Fries, Léveillé, Berkely, and Corda: the zoospores of Vaucheria had been seen, and the conjugation of Spirogyra was known. Thuret and Nägeli were at work: Hofmeister was publishing his work on the embryology of the Phanerogams and Cryptogams: Von Mohl was creating a new school of vegetable anatomy.

Surrounded by these influences, De Bary was working at the structure and development of the Fungi causing "Rusts" and "Smuts," and in 1853 he published his first book on this subject: Thuret observed the details of the fertilization of Fucus in the same year.

De Bary was also occupied with the Algæ, and in 1854 published his observations on *Œdogonium* and *Bulbochæte*: Pringsheim's papers on *Vaucheria*, *Œdogonium*, *Saprolegnia*, and *Coleochæte* appeared in 1855-58. The great botanical question of the day was the development of the lower Cryptogams.

Then came De Bary's researches on the *Conjugateæ*, published in 1858, where the essentials of sexual reproduction are described with wonderful accuracy; and this was followed by his observations on the germination of *Lycopodium*, a piece of work so good that although we have only come into possession of most of the remaining facts quite recently, his old figures have been found worth reproducing.

But, as is well known, De Bary abandoned this newer pursuit of the green plants to return to his earlier love, the Fungi; and from about 1860 onwards he sent forth memoirs and books into the world of a nature to shake the tottering hypotheses of the day to their foundations, building up in their place the beginnings of what is rapidly becoming a mighty and coherent superstructure.

Until about 1850 little was known of Fungi beyond the mycelium and spores of the larger forms. The Tulasnes were at work, and had described several of the "Rusts," &c., before De Bary's book came out, and by 1853 the development of the ergot of rye had been observed. Then followed their brilliant descriptions of the development and germination of the spores of *Cystopus, Puccinia, Tilletia, Ustilago*, and in 1861-65 Tulasne's "Selecta Fungorum Carpologia" appeared.

De Bary was already bringing forward the methods which distinguish his work so eminently from the anatomical method of his predecessors, and by 1863 he had not only cultivated many forms of Fungi, and repeatedly seen the sexual organs of the *Peronosporeæ*, but he showed that the fructification of the *Ascomycetes* is also to be traced back to the interaction of sexual organs. These may be regarded as the starting-points of the long series of researches into the sexuality of the Fungi which have already led to such remarkable results, and with which the names of De Bary and his school are so intimately associated.

In 1864, De Bary published the second edition of his book on the *Myxomycetes* (the first edition was in *Zeitschr. für Wiss. Zool.* 1859), and we ought to point out that the first edition of this work, coming at the time when the observations on zoospores by A. Braun, Thuret, Nägeli, Pringsheim, and De Bary himself, were astounding the botanical world, helped much towards clearer conceptions regarding the "sarcode" of the zoologists, and the protoplasm of the botanists.

This year (1864) also saw the first number of the celebrated "Beiträge zur Morph. u. Phys. d. Pilze," and in 1865 were produced the startling results of his further cultures of parasitic Fungi, in which he showed how—by regarding a parasite as an organism to be cultivated on its proper medium, just as we regard wheat as an organism to be grown on suitable soil—its life-history can be followed without those large breaks in continuity which render so much of the anatomical evidence worthless. By means of these researches De Bary proved the entrance of the parasitic Fungus into the host, and its progress in the tissues, so conclusively that any doubts still lurking on the main subject were for ever dispersed. The importance of these results cannot be rated too highly: they not only entirely altered the position of the agriculturist towards his fungoid enemies, but they introduced a new era in medicine. Their bearings on science were simply beyond valuation. From this point onwards the continuous observations of cultures under the microscope became extensive; and in the hands of those who were not too readily deterred by the technical difficulties and the laborious patience of such researches, there sprang up the beginnings of that knowledge of the diseases of plants which is now taking shape under the action of workers trained by De Bary himself.

Nor was this all. The startling facts of heterœcism were at the same time put before the world, and on such evidence that none could reject the phenomenon: De Bary proved that the so-called *Æcidium* of the Berbery is only a phase in the life-history of the *puccinia* of the rust of wheat. The repeated confirmation of this in later years, and the numerous similar cases which have been discovered since, sufficiently attest the accuracy of the original work; while its practical importance is obvious.

In 1866 was published the first edition of the "Morphologie und Physiologie der Pilze, Flechten, und Myxomyceten," a book which gave definiteness to the scattered knowledge of these organisms, and enabled the scientific world to see clearly the remarkable power of the man. His unflinching honesty and rigorous self-criticism and modesty had already attracted the attention of all who came in contact with him or his work; now, however, was seen the marvellous grasp of details, and the power of logical generalization which he possessed, and thenceforward the name of De Bary was associated with the leadership of the modern school of biologists he was himself creating.

As evidence of his untiring industry, it may be pointed out that not only did he publish the second number of the "Beiträge zur Morph. und Phys." this year, but he had already taken in hand that monument of laborious investigation and critical reading, the "Comparative Anatomy of the Phanerogams and Ferns," which was not finished until 1877. The years 1869, 1870, and 1871 show indications of his new labours—undertaken, it should be mentioned, because the original plan had been interfered with—in articles in the *Botanische Zeitung*, on the epidermis, on *Cycads*, &c. Nevertheless the third number of the "Beiträge" appeared in 1870, full of new work, and important, on the *Erysipheæ* and *Ascomycetes*.

During 1874 and 1875 he published two papers on the fertilization and germination of *Chara*, and a memoir on *Protomyces*. In 1877 was published his and Strasburger's joint memoir on *Acetabularia*, and the book above referred to—the "Comparative Anatomy of the Ferns and Phanerogams"—was finished. The influence of this work has been enormous: criticism has been cast on the plan and mode of treatment, but probably all botanists capable of judging are unanimous in praising its extreme accuracy, justice, and completeness. 1878 and 1879 saw the publication of the essays on apogamy, and on symbiosis, two bright and suggestive papers, which have had a wide influence on succeeding work, and which connect De Bary's name paternally with new doctrines in biology.

In 1881 he was busy with the promulgation of his new facts and deductions in connection with the *Perono-*

sporeæ, and the phenomenon of apogamy in the Fungi. In addition to articles in the *Botanische Zeitung* on the classification of the Thallophytes generally, and of the Fungi in particular, he published extended and important observations on the *Saprolegnieæ* and *Peronosporeæ* (the fourth number of the "Beiträge zur Morph. u. Phys."), and the philosophical scheme of classification of the Fungi which forms the basis of our present system. Space will not admit of our referring further to his other memoirs, and it is impossible to even mention the numerous illuminating ideas and suggestions which are scattered through his papers, for we must proceed to the passing enumeration of his last two books, either of which would have sufficed for the reputation of an ordinary great man.

In 1884 was published his "Comparative Morphology and Biology of the Fungi, Mycetozoa, and Bacteria," and the best idea of De Bary's influence can be obtained by comparing this work with his "Morph. u. Phys. d. Pilze, Flechten, u. Myxomyceten," published eighteen years previously.

Apart from the development of his own ideas, and the accumulated results of his investigations, the reader who knows something of the names in the footnotes and references will find proofs of what an active school of investigators has been developed under the direct influence of this able philosopher; and he may gather that the sympathetic advice of the great master aided many of them in turn in the unravelling of difficult biological problems.

In 1885, De Bary brought together a series of lectures on Bacteria, since published in the form of a book: it is in his best style, and brings before the reader by far the clearest trustworthy general account of this astonishing and fruitful subject. Here, as everywhere, to take a subject in hand was to aid it: had De Bary done no more for "bacteriology" than observe and clearly describe the development of the spores of *Bacillus megaterium*, his influence would have been felt; and the student is especially indebted for his careful sifting of the literature, and his suggestive indications.

One of his latest efforts was on the subject of infection, particularly with reference to certain *Pezizæ* and Sclerotia: he placed firmly on record the discovery that some of these Fungi may be harmless saprophytes until they have been cultivated—educated up to a higher degree of power—and then they can enter into and destroy a living host, which resisted them previously.

It should also be remembered that he was for many years editor of the *Botanische Zeitung*, and lent his aid to the forwarding of numerous botanical projects.

The above sketch may serve to convey some idea of the labours of the great Strassburg botanist. But, although they give a glimpse of the specialist's results, they afford no insight into his keen appreciation of all good work; of his humorous and never malicious disposition, in the laboratory, and in his writings; and of his sharp, but always just, criticism of anything pretentious. Nor is it possible to enter here into his abundant knowledge of species: he was one of the first to grasp Darwin's teachings, and perhaps never misapplied them. His close acquaintance with species and even local varieties of the plants around Strassburg, at any rate, could only be known to those who have walked with him; and the delight of those walks in Alsace!

As a lecturer he was not brilliant: he appeared shy and nervous when on the daïs, but in spite of his low voice and restless fingers he kept his hearers interested, and always taught clearly. Quaint he often was, in speech and manner, but the impressive truthfulness of his nature, the earnestness of his teaching, and the absence of any striving after effect, gave to his very quaintness a charm and dignity the influence of which will never be forgotten so long as his pupils live.

NOTES.

WE print to-day a leading article on "Odium Medicum." As the questions to which it relates have already been fully discussed in the *Times*, it may be well to state that we do not intend to publish any correspondence on the subject.

SOME time ago the Australian Governments, through Sir Graham Berry, represented to the Home Government the fact that in their opinion much good might be done by an "Antarctic *reconnaissance*," preliminary to an expedition for the thorough exploration of the Antarctic regions. In order that this suggestion might be carried out, the Australian colonies offered to contribute £5000, on condition that a like sum should be given by the mother country. The proposal was supported by the Colonial Office, by the Royal Society, and by the Royal Geographical Society; nevertheless, the Treasury has announced that it does not see its way to the granting of an Imperial contribution. The objects to be attained do not seem to it to justify the payment of even so small a sum as £5000. There will, of course, be much disappointment in the Australian colonies, but it may be hoped that the idea of a joint Antarctic Expedition will not be abandoned. Perhaps a larger scheme than the one which has just been rejected would have had a better chance of success.

THE scientific education of the mining population of Cornwall was for many years in the hands of the Miners' Association of Cornwall and Devon—an institution founded in 1859, at the suggestion of the late Mr. Robert Hunt, F.R.S. Some time ago this body was amalgamated with another Cornish institution, and the united organization took the name of the Mining Association and Institute of Cornwall. A movement has just been set on foot for increasing the efficiency of this Association by the formation of a Museum of Mineralogy, to be established at Redruth, or elsewhere, in the heart of the great tin and copper mining district. It is held that such an institution will in no way interfere with the existing museums in the county—such as those of the Royal Institution at Truro, and of the Royal Geological Society at Penzance. The new museum, instead of seeking to exhibit attractive specimens, will be essentially practical and educational—a place for the earnest student rather than for the casual visitor. It will endeavour to collect characteristic samples of ores, and typical specimens of such other minerals as are of interest to the miner or to the geologist. In recognition of the services which Mr. Robert Hunt rendered to Cornwall by his persistent advocacy of the necessity of giving the young miners a scientific training, it is proposed that the new museum shall bear his name. The Committee appeals for contributions, either in money or in minerals, and for suggestions as to the development of the scheme. Communications should be addressed to Mr. T. C. Peter, Town Hall, Redruth.

MR. J. E. HARTING has been appointed Librarian and Assistant Secretary to the Linnean Society at Burlington House, in the place of Dr. Murie, resigned. Mr. Harting has for some years past been engaged in fulfilling the duties of Zoological Librarian at the Natural History Museum, South Kensington, where he has organized what is now the best zoological library in this country, although possibly not the largest in regard to the number of volumes. The new appointment has been made opportunely at the expiration of the Government grant for the purchase of books at South Kensington, and has given general satisfaction.

THE forty-first annual general meeting of the Institution of Mechanical Engineers will be held on Thursday, February 2, and Friday, February 3, at 25 Great George Street, Westminster. The chair will be taken by the President, Mr. Edward H. Carbutt, at half-past 7 p.m. on each evening. The discussion on Mr. John Richards's paper, on "Irrigating Machinery

on the Pacific Coast," will be resumed. The following papers will be read and discussed, as far as time permits:—"On the Position and Prospects of Electricity as applied to Engineering," by Mr. William Geipel, of Edinburgh; "Third Report of the Research Committee on Friction: Experiments on the Friction of a Collar Bearing."

THE 1888 Conference of the Camera Club, the central institute for amateur photographers, will be held in the theatre of the Society of Arts on Tuesday and Wednesday, March 13 and 14, under the presidency of Capt. W. de W. Abney, F.R.S.

THE eighth annual general meeting of the Essex Field Club will take place at the Public Hall, Loughton, Essex, on Saturday evening, January 28, at seven o'clock. Mr. T. Vincent Holmes will deliver the annual Presidential address, taking as his subject "The Subterranean Geology of South-Eastern England."

A PUBLIC Conference on the Sanitary Registration of Buildings Bill will be held at the Society of Arts, John Street, Adelphi, on Saturday, February 4. The chair will be taken at 4 o'clock by Sir Joseph Fayrer, F.R.S.

A NATIONAL Hydrographical, Meteorological, and Climatological Congress is to be held at Madrid in February.

THE American Society of Naturalists held its annual meeting in the Peabody Museum, New Haven, on December 27 and the two following days. *Science* explains that this Society, composed of professors and specialists, leaving to other scientific associations the function of presenting and discussing results, devotes itself to the publication of new methods, improved apparatus, and aids to science-teaching. The work of the Society falls into two sections—biology and geology—and a day of each meeting is devoted to each of these topics, while the third day is given over to a general discussion on some attractive subject. The attendance at the recent meeting was large, and, according to *Science*, the proceedings were both interesting and profitable.

THE *Monthly Weather Review*, published by the Chief Signal Officer of the United States for October 1887, contains a discussion of the movements of high barometer areas over the North Atlantic Ocean for the year 1885. Fifty-two well-defined areas passed off the coast, of which seven traversed the ocean to Europe, and three moved north-easterly, to the vicinity of Iceland. The average time occupied by the fifty-two anticyclones in advancing from the 90th meridian to the coast was about one day and a half, this rate of progression being considerably greater than the average velocity of cyclonic areas over that region. These areas of high pressure have an important influence on the paths of storms. During October 1887 the paths of sixteen depressions are also traced; four advanced eastward over Newfoundland, one of which traversed the ocean from coast to coast.

THE Meteorological Council have published the observations taken at stations of the second order during the year 1883 (218 pp. large 4to). Observations taken twice a day are printed *in extenso* for thirty stations, and monthly and annual summaries and extremes for forty-four stations. The positions of the stations are shown upon a key-map, but the map also shows that considerable districts in the west of Scotland and Ireland, and even on the east coast, for instance between Dundee and Seaham, are still unrepresented. The barometer observations (reduced to mean sea-level) are given to the nearest ·01 inch, instead of the ·001 inch as heretofore. There is also a useful summary of the hours of bright sunshine for the stations which are furnished with sunshine-recorders, but the yearly values are not calculated.

WE have received a sheet on which are three photographs of the total eclipse of the sun, August 19, 1887, taken at Yōmeiji-yama (long. 138° 59′ 23″ E., lat. 37° 37′ 13″ N., alt. 115 metres), Echigo, Japan, by M. Sugiyama, the observer of the Tokio Observatory, under the direction of I. Arai, the Director of the Tokio Observatory and the chief of the Expedition. The photographs were taken in the following order: L.M.T. 3h. 40m. 36·5s. (1m. 8s. after beginning of totality); L.M.T. 3h. 41m. 25·4s. (1m. 57s. after beginning of totality); L.M.T. 3h. 42m. 6·2s. (34s. before end of totality). In sending us these photographs, Mr. I. Arai writes to us:—"While bad weather prevented nearly all the observations at other stations in our country, I was very fortunate, my station being entirely free from clouds, at least during the totality. But I regret to inform you that, as we were not equipped with complete instruments, and the telescope used was only of small size and not sufficient for photographic purposes, the result was not very satisfactory, because some of the coronal rays, extending outside of the field of the telescope, do not appear in the photographs. I did not, however, like to make the least modification, neither in size, nor in shape, believing that it would be best to leave the actual phenomena just as represented by the photographic apparatus."

SEVERE earthquakes are reported from Ontario and Quebec on January 11, but no damage was done. Shocks are also reported from Columbia (South Carolina), Sammerville, and Charlestown. According to a telegram sent from New York on January 23, three shocks had occurred at Newburyport, in Massachusetts.

MESSRS. MACMILLAN have arranged to publish in their "Student's Series" a new biological text-book, "Lessons in Elementary Biology," by Prof. T. Jeffery Parker, of the Otago University, New Zealand. The book will be written on a modification of the "type" system, the earlier chapters consisting of detailed accounts of the morphology, physiology, and life-history of selected examples of the lower organisms. Briefer accounts of important types of the higher animals and plants will be given, but, as the work is intended for the study and not for the laboratory, it will not be necessarily limited to readily accessible forms, and the plan will sometimes be adopted of omitting certain points of structure, development, &c., which from their complexity or aberrant character are unsuited to an elementary work. The book will be written throughout in such a way as to bring clearly before the student the fundamental principles and generalizations of biology, and will be fully illustrated. It is hoped that it may serve to supplement the lecture-notes of a student attending an ordinary junior University course of biology, and, in the case of one working independently, to supply the connected narrative which is not readily obtained in suitable form either in a laboratory manual or in the ordinary text-books of zoology and botany.

THE "Zoological Record" for 1886 has just been issued. For sixteen years the annual volume of this most useful work was issued by the "Zoological Record" Association, which was aided by grants from various sources. At the close of 1886 the Association failed to obtain the renewal of some of these grants; and, being unwilling to carry on the publication of the "Record" any longer, it came to an agreement with the Zoological Society, by which the task was undertaken. The Council of the Zoological Society appointed a Select Committee to superintend the new enterprise, and Mr. F. E. Beddard was made editor. In the preface to the present volume Mr. Beddard explains that the only change he has made is the introduction of a section devoted to general subjects. This includes text-books and works of a general nature, many of which are again recorded under the several groups with which they are more especially concerned.

UNDER the title "A Year's Insect-Hunting at Gibraltar," there appears in the January number of the *Entomologist's Monthly Magazine*, a valuable paper by Mr. James J. Walker,

on the entomology of Gibraltar, concerning which subject next to nothing had previously been written. Mr. Walker, as an officer of H.M. gunboat *Grappler*, stationed there, had ample opportunities for studying the insect-fauna. His observations are mainly confined to *Lepidoptera* and *Coleoptera*. He says there is scarcely a day throughout the year on which butterflies may not be found; and he enumerates fifty-five species for the limited district, thirty of which have occurred on the isolated Rock itself. *Coleoptera* are very numerous, and he has already found 900 species, and is almost daily adding to the number. Apart from its purely entomological interest, the introductory portion is of great value, being a lucid *résumé* in a few pages of the topography of the Rock and the immediate neighbourhood, with sketches of the chief botanical, zoological, geological, and meteorological features, not forgetting the Barbary apes, which, reduced a few years ago to less than a dozen individuals, are now so numerous as to cause serious depredations in the gardens.

WE have received the volume for 1886 of the Journal and Proceedings of the Royal Society of New South Wales. Among the contents may be noted the Presidential Address by Prof. Liversidge, F.R.S.; description of an unrecorded Ardisia of New Guinea, by Baron von Mueller, F.R.S.; a comparison of the dialects of East and West Polynesian, Malay, Malagasy, and Australian, by the Rev. G. Pratt; preliminary notes on some new poisonous plants discovered on the Johnstone River, North Queensland, by T. L. Bancroft; notes on the process of polishing and figuring 18-inch glass specula by hand, and experiments with flat surfaces, by H. F. Madsen; notes on the theory of dissociation of gases, by Prof. R. Threlfall.

A LARGE number of new aromatic fluorine substitution products have recently been prepared by Drs. Wallach and Heusler (*Liebig's Annalen*, Band 243, Heft 1 and 2), the properties of which point to some interesting conclusions regarding the physical nature of fluorine itself. It is found that in all cases the specific gravity of a compound is raised by the introduction of fluorine instead of hydrogen. Thus while benzene at 20° has the specific gravity 0·8846, fluorbenzene, C_6H_5F, at 20° possesses a specific gravity of 1·0236. But, on the other hand, the substitution of fluorine is found to have a remarkably small effect in raising the boiling-point; for instance, fluorbenzene enters into ebullition at 85° C., a temperature only 5° higher than that of boiling benzene. What is, however, still more interesting is the fact that the difference between the boiling-points of corresponding iodine and bromine substitution products, and again between those of bromine and chlorine is smaller than that between the substitution derivatives of chlorine and fluorine. Whilst this difference of boiling-point between corresponding bromides and chlorides amounts to 20–23°, that between chlorides and fluorides approaches 40°. This fact, coupled with the small influence which the substitution of fluorine exerts upon the boiling point, indicates the interesting probability that the boiling-point of free fluorine itself lies very much below that of chlorine (–33°·5), and that fluorine much more nearly approaches the volatility of hydrogen. Indeed, it appears likely that fluorine is one of the so-called permanent gases, and might form a worthy object for the attentions of those who have been so successful in inducing the other "permanent" gases to reveal their boiling-points; the difficulties in the way would of course be immense, but, in face of what has been done, are not perhaps insuperable. Under all circumstances fluorine attaches itself to carbon with far greater tenacity than any of the other halogens, as was clearly shown by leaving one of the new fluorides, bromfluorbenzene, C_6H_4BrF, in cold ethereal solution in contact with metallic sodium. After eight days a considerable quantity of sodium bromide had formed, but not a trace of the fluoride of sodium. The fluor-compounds themselves form a most valuable contribution to organic chemistry, and fill up a gap which has long been noticeable in the literature of the subject.

MR. J. A. CROWE, Her Majesty's Commercial Attaché for Europe, reports to the Board of Trade that the French Legislature has recently passed a law enacting that a prize will be given to the discoverer of a simple and practical test to ascertain the presence in spirits and alcoholic drinks of substances other than pure and ethylic alcohols. The conditions under which the award is to be made will be determined by the Academy of Sciences of the French Institute.

IN the last number of the *Zoologischer Anzeiger*, Dr. Otto Zacharias earnestly recommends the establishment of a zoological station on a German lake for the observation and study of the freshwater fauna.

THE other day three ladies in India received the degree of B.A.,—two at the University of Calcutta, and one at the University of Bombay.

A SEAM of good coal is reported to have been discovered in Cashmere. An officer of the Indian Geological Survey is to be sent to examine it.

RECENTLY an elk was shot in Galicia. It is now 130 years since the last of these animals was killed in Austria. It is believed that the one referred to had come from Lithuania.

IT is generally believed that the Polar bear cannot be tamed. Last autumn, however, a Norwegian skipper brought one of these bears with him from the Arctic Sea to Tromsö, and it has become quite tame. The bear plays like a dog with the crew of the vessel, and follows its master everywhere. It is nearly full grown.

THE Spitzbergen whale-fisheries have been more remunerative during the last two years than at any time during the past quarter of a century. Last year 1311 animals were killed. The whalers are English, Russian, and Norwegian.

A MAGNIFICENT gift has lately been received by the Ethnological Museum at Leipzig, from Dr. Alphonse Stübel (Dresden), Dr. Wilhelm Reiss (Berlin), and Consul-General Benedix Koppel (London). It consists of a rich collection of articles illustrating the culture and industry of ancient and modern South American races. The collection is divided into two parts: the first being objects belonging to the period before the Spanish conquest, the second being modern. There are many figures, vessels, weapons, and implements of stone and clay, found in the old Columbian, Bolivian, and Peruvian tombs, as well as ancient silver, copper, and bronze ornaments from Ecuador and Peru. The Columbian antiquities, and the ancient gold objects of the Chibchas, are specially noteworthy.

THE additions to the Zoological Society's Gardens during the past week include two Snow Finches (*Montifringilla nivalis*), European, presented by the Lord Lilford; two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, two Pale-headed Parrakeets (*Platycercus pallidiceps*) from North-East Australia, presented by the Hon. Stormont Finch-Hatton; an African Buzzard (*Buteo desertorum*) from Africa, presented by Mr. Sydney H. Carr, four Barbary Turtle Doves (*Turtur risorius*) from North Africa, presented by Mr. John Biehl; two Herring Gulls (*Larus argentatus*), British, presented by Mr. Thomas A. Cotton; a Common Barn Owl (*Strix flammea*), British, by Mr. Hugh Bromley; a Moorish Gecko (*Tarentola mauritanica*) from France, presented by Mr. J. C. Warbury; two Viscachas (*Lagostomus trichodactylus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CAPE OBSERVATORY.—The second portion of the data upon which the forthcoming Cape Catalogue for 1885 will be founded has recently appeared. The first portion, containing the results of the meridian observations made during the years 1879, 1880, and 1881, was published by Dr. Gill some time ago, and the present volume gives the results from the beginning of 1882 to February 8, 1885, when, the programme for the observation of the fundamental stars of Schönfeld's *Durchmusterung* —which stars will form the most important part of the Catalogue —having been completed, further work with the transit instrument was suspended. An additional reason for the interruption of the meridian observations lay in the desirability of re-polishing the object-glass, and of replacing the micrometer screws of the circle microscopes, which were of gun-metal, by steel screws. The investigation of the errors of the screws used in the present observations forms the most important portion of the introduction, for the effect of wear upon them has attracted Dr. Gill's special attention, and has already formed the subject of a lengthy paper by him in the *Monthly Notices* of the R.A.S., vol. xlv. The transit instruments of the Cape and Greenwich Observatories are almost exactly alike in construction; it is therefore interesting to note that there are evident differences in their behaviour; thus the mean horizontal flexure of the Cape instrument, as determined by the collimators, amounts to nearly half a second—0″·462—whilst that of the Greenwich telescope is almost insensible.

The introduction is followed by 144 pages giving the separate determinations of the various instrumental corrections, the readings of the transit-circle thermometers, &c. The ledgers and catalogues for the years 1882, 1883, and 1884 occupy the remaining 400 pages, the catalogues for the three years containing respectively 863, 444, and 1301 stars, reflex or sub-polar observations of stars being counted separately.

THE PARALLAX OF MARS.—We have received a letter from Mr. C. G. Stromeyer, calling attention to the fact that Mars is stationary on March 4, and urging the desirability of determining its parallax by the diurnal method, the rather that it will then be near two sixth-magnitude stars, as will be seen by the following positions:—

	Mag.	R.A. h. m. s.	Decl. ° ′ ″
95 Virginis	6	14 0 48	8 46 47 S.
94 Virginis	6	14 0 22	8 21 27 S.
Mars		13 56 16	9 2 20 S.

Unfortunately, however, the parallax is small—only 11″·3, and only part of this is practically available for the diurnal method, as the planet cannot be observed through a longer period than eight hours at the utmost.

THE LONGITUDE OF ODESSA.—The *Astronomische Nachrichten*, No. 2820, gives the result of the determination, by Dr. E. Becker and Prof. Block, of the difference of longitude between Berlin and Odessa, which was carried out in July and August 1876 by the telegraphic method. The deduced distance in longitude of the centre of the axis of the Repsold meridian-circle of the Odessa Observatory to the east of the centre of the great dome of the Berlin Observatory is given as 1h. 9m. 27·29s.

THE WINKLER OBSERVATORY.—Herr Winkler notifies, in No. 2821 of the *Astronomische Nachrichten*, the transference of his private observatory from Gohlis, near Leipzig (N. lat. 51° 21′ 35″·1; long. E. from Greenwich, 0h. 49m. 29·65s.), to the neighbourhood of Jena. The growth of the city of Leipzig rendered the old site no longer a favourable one for observation. The transit-instrument and small 4-inch refractor are already temporarily mounted. The co-ordinates of the new observatory are taken at present as being N. lat. 50° 55′ 35″·6; long. E. from Greenwich, 0h. 46m. 20·8s. Herr Winkler publishes at the same time some observations of occultations and eclipses of Jupiter's satellites made in the first half of 1887, which were the last observations made at Gohlis.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *JANUARY* 29—*FEBRUARY* 4.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

***At Greenwich on January* 29**

Sun rises, 7h. 45m.; souths, 12h. 13m. 19·9s.; sets, 16h. 41m.: right asc. on meridian, 20h. 46·1m.; decl. 18° 0′ S. Sidereal Time at Sunset, 1h. 14m.

Moon (at Last Quarter on February 4, 19h.) rises, 16h. 20m.*; souths, 0h. 15m.; sets, 8h. 1m.: right asc. on meridian, 8h. 46·0m.; decl. 17° 56′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury	8 16	12 46	17 16	21 18·8	17 39 S.
Venus	5 20	9 23	13 26	17 55·5	21 47 S.
Mars	23 32*	5 0	10 28	13 31·9	7 2 S.
Jupiter	3 13	7 30	11 47	16 1·6	19 45 S.
Saturn	15 51	23 45	7 39*	8 19·8	20 6 N.
Uranus	23 1*	4 33	10 5	13 4·5	6 9 S.
Neptune	11 29	19 8	2 47*	3 41·6	17 54 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Jan.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
29	7 Leonis	6½	17 41	17 55	332 298
29	ψ Leonis	6	21 13	21 24	320 300
Feb. 3	80 Virginis	6	3 33	4 44	38 256

Feb. 3 ... 8h. ... Mars in conjunction with and 2° 50′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Jan. 30,	21 0	*m*
R Canis Majoris	7 14·5	16 12 S.	,, 30,	3 8	*m*
U Monocerotis	7 25·5	9 33 S.	,, 30,		*M*
U Hydræ	10 32·0	12 48 S.	,, 29,		*M*
R Crateris	10 55·1	17 43 S.	Feb. 2,		*M*
δ Libræ	14 55·0	8 4 S.	Jan. 29,	19 25	*m*
			Feb. 1,	3 16	*m*
U Boötis	14 49·2	18 9 N.	Jan. 30,		*m*
U Coronæ	15 13·6	32 3 N.	Feb. 1,	4 44	*m*
S Serpentis	15 16·4	14 43 N.	Jan. 31,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 29,	23 57	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Feb. 2,	5 0	*m*
U Aquilæ	19 23·3	7 16 S.	,, 4,	5 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	Jan. 29,	19 0	*m*
			Feb. 2,	4 0	*M*
Y Cygni	20 47·6	34 14 N.	Jan. 31,	20 22	*m*
			Feb. 1,	20 15	*m*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

GENERAL PRJEVALSKY has begun to print his narrative of his fourth journey in Central Asia. It is expected to appear in May, and we may hope that it will find an English translator.

WE are glad to learn that the French explorer of the Gran Chaco, M. Thouars, is safe. The Bolivian Government succeeded in rescuing him from a perilous position among hostile Indians.

A SCOTCH merchant captain, Mr. Strachan, has just returned from New Guinea, many hitherto unexplored parts of which he seems to have visited. It is expected that he will be able to give information that will seriously modify the cartography of the Fly River region. He maintains that the forests in New Guinea are confined to a fringe along the banks of the rivers, and that the bulk of the interior is covered with grass. Captain Strachan has brought home with him a young Papuan boy.

THE steamer *Essex*, of the United States Navy, has been making a series of soundings between Cape Guardafui and Ceylon. In the Indian Ocean, between 60° and 70° E. long., a uniform depth of about 2000 fathoms is almost constantly met with, gradually decreasing as the coast is approached. The greatest depth met with was 2705 fathoms, off the coast of Africa, 160 miles from Cape Guardafui. To the east of this

maximum, the sea-bed rises suddenly to a depth of only 857 fathoms below the surface.

THE new part of the *Mittheilungen* of the Hamburg Geographical Society contains several papers of interest. Dr. Sievers concludes the long series of papers describing the results of his journeys in Venezuela with some remarks on his original route map of the Venezuelan Cordilleras, which are accompanied by an admirable reproduction of this map. In addition to this, Herr Frohberg arranges and discusses the barometric results obtained by Dr. Sievers. Dr. Zintgraff describes the Lower Congo from Banana to Vivi, and insists on the importance of the Congo for the exploration of the region behind the German Cameroons protectorate. Herr Weisser gives a fairly complete account of German New Guinea and the Bismarck Archipelago; and Herr Hernsheim does the same for the Marshall Islands.

As a supplement to the Indian Survey Report for 1885-86, there has just been issued the narrative of the journey of a native explorer, M—H, through Eastern Nepaul into Southern Tibet, as far as the town of Dingri, and westwards and southwards through Central Nepaul. M—H has succeeded in rectifying in many points existing information on the hydrography of the region traversed, and gives many useful notes on its physical geography and its flora, as well as on the people.

OUR ELECTRICAL COLUMN.

THE additional facts added to our knowledge of electricity in 1887 are not very numerous, but the impetus given to its practical applications was very encouraging. One of the most important scientific discoveries was that of Prof. J. J. Thomson, which formed the subject of the Bakerian Lecture, viz. that sparks in tubes dissociated iodine, bromine, and chlorine. In iodine the dissociation produced at 214° C. was as much as that effected directly by Victor Meyer at 1570° C.

PROF. EWING showed that there was apparently no limit to the magnetization of iron in strong magnetic fields when we increased the magnetizing force, and Prof. Roberts Austen showed that it was impossible to separate the elements of alloys by means of electric currents.

IMMENSE improvements have been made in the construction of dynamos, motors, accumulators, and secondary generators, and in consequence electric lighting and working of railways and tramways are upon a commercial and useful stage. Many other causes besides restrictive legislation have retarded electric lighting in England, but there are now many signs that this useful industry is in more senses than one about to commence a very bright career.

SEVERAL useful constants have been added to our notebooks during the past year. Dr. John Hopkinson is pursuing his examination of the specific inductive capacity of oils and other liquids.

MR. PREECE has determined the coefficient of self-induction of straight iron telegraph aërial wires to $0{\cdot}005 \times 10^9$ centimetres per mile, while that of copper wire is practically *nil*. He has also measured the current which will just actuate a Bell telephone, and he found it to be 6×10^{-13} ampere.

THE application of powerful electric currents to smelting, as in the Cowles process for producing aluminium, and to welding, as proposed by Elihu Thompson, is gaining rapid progress, while the use of enormous dynamos for the deposition of pure copper from impure ores is gaining ground with giant strides. Messrs. Bolton, at Widnes, and Messrs. Vivian, as well as Messrs. Lambert at Swansea, are each depositing from forty to fifty tons of copper per week by currents of from 5000 to 10,000 amperes.

THE Society of Telegraph-Engineers and Electricians has decided to change its title to that of the Institution of Electrical Engineers—a change for the better. Mr. Graves, the new President, gave an exceedingly interesting address on the industrial importance of electricity, and he brought out the remarkable fact that there are at least 300,000 persons in the United Kingdom depending upon electrical industry for their daily bread.

SOME of our prominent workers in the field of electricity, such as Lord Rayleigh, Sir William Thomson, and Prof. Hughes, are conspicuous by their absence during the past year, although the two former have been by no means idle in other directions.

A NOTE ON VALENCY, ESPECIALLY AS DEFINED BY HELMHOLTZ.[1]

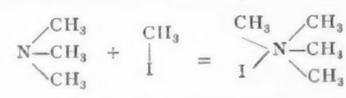
VERY little has been either said or written of late on the subject of valency—not because the topic is admitted to be exhausted, nor because our views can be regarded as reposing on a fixed basis of fact, but more I believe on account of the feeling being almost universally entertained that little is to be gained by continuing the discussion from our present standpoint.

My purpose in this note is to call attention to the extreme importance of reopening the discussion on account of the intimate bearing that it has on the work in which the Electrolysis Committee, jointly appointed by Sections A and B, are now engaged; and to urge that it is time that the gage thrown down by Helmholtz in the Faraday Lecture (Chem. Soc. Trans., 1881, p. 277) was uplifted by chemists.

We are told by Helmholtz that it is a necessary deduction from the fundamental law of electrolysis established by Faraday, that definite, as it were atomic, charges of electricity are associated with the atoms of matter; that, in fact, a monad bears a single charge, a dyad two, a triad three; and that when combination occurs the charges are still retained by the atoms but neutralize each other—"the atoms cling to their charges, and opposite electric charges cling to each other." I cannot help thinking, however, that Helmholtz deprives his statement of much of its force and simplicity by adding: "But I do not suppose that other molecular forces are excluded, working directly from atom to atom." He is led to do this apparently by being aware of the distinction which it is usual to draw between atomic and molecular compounds. The attempt should at all events be made—and in my paper on "Residual Affinity" I have already ventured the first step —to include both classes of compounds, molecular as well as atomic, in the discussion; indeed it is somewhat difficult to reconcile the passage above quoted with the following statement which occurs previously in the lecture: "The law of the conservation of energy requires that the electromotive force of every cell must correspond exactly with the total amount of chemical forces brought into play, not only the mutual affinities of the ions, *but also those minor molecular attractions produced by the water and other constituents of the fluid.*" The italics are mine. But if the "minor molecular attractions" contribute to the electromotive force of the cell, then conversely these also will have to be overcome in effecting electrolysis, and are as much to be reckoned as are the "mutual affinities of the ions"!

It is obvious that if it should prove possible to decide what number of charges are necessarily associated with any particular atom, the conception of valency will have acquired a definiteness which cannot possibly be attached to it as long as the views that have hitherto guided us are adhered to. A decision must involve the discussion of the question of the existence of molecular as distinct from atomic compounds.

To cast the apple of discord without further preface, I would direct attention to the insufficiency of the evidence on which it is usual to rely as proof that nitrogen, for example, is a pentad; nay more, I would assert that this very evidence should be interpreted as proof that nitrogen is not a pentad. It is commonly held that the behaviour of the alkyl tetra-substituted derivatives of ammonium is such as to negative the idea that these are "molecular compounds" of triad nitrogen, and that it must be assumed that the elements of the binary compound which are added to the ammonia derivative are *distributed* in the ammonium derivative; for example, that in the formation of tetramethylammonium iodide from trimethylamine and methyl iodide the methyl and iodine of the iodide part company and separately attach themselves to the nitrogen, thus:—



But I contend that the properties of tetramethylammonium iodide and hydroxide prove that such is not the case: the iodide, it is well known, can be boiled for hours with the strongest caustic potash solution without undergoing change; there is not a single

[1] A Paper read by Prof. Henry E. Armstrong, F.R.S., in Section B of the British Association at Manchester. Communicated by the Author.

case on record, however, of any haloid compound other than an alkylic compound behaving in this manner; the chlorides, bromides, and iodides of every element except carbon are almost at once converted into hydroxides by such treatment, and a nitrogen iodide would surely be acted on. The behaviour of the iodine is much more nearly that of iodine in methyl iodide, and, it may be said, exactly that of the iodine in iodobenzene; indeed it would seem that in the alkyl-ammonium haloid compounds the halogen is always less easily displaced by the action of alkalies than it is in the parent haloid alkylic compound.

The remarkable resemblance of the tetra-substituted ammonium hydroxides to potassium hydroxide has led to their being regarded as in every respect analogous to this latter, and would appear to preclude the idea that they are molecular compounds of an alcohol with an ammonium derivative. But attentive consideration of their properties will suffice, I think, to show that the apparent discrepancies are not only explicable, but that they actually support the molecular compound hypothesis. Thus it might be said to be improbable that tetramethylammonium hydroxide should behave as a powerful base, and have the same heat of neutralization as potassium hydroxide, if methyl-alcohol were one of its proximate constituents; but it is to be remembered that the salt which results from the action of an acid on methyl-alcohol is liable to suffer reconversion into the alcohol by the action of the water produced in the interchange; also that in many cases the methyl salt is insoluble in water, or nearly so. The heat developed on neutralizing methyl-alcohol therefore falls far short in amount of that which would be evolved if the interchange were complete, and if the product were capable of interacting with water, and perhaps also with itself in the way that apparently is possible in the case of metallic salts. In the case of the tetramethylammonium hydroxide, the action of acids is total as the change is irreversible, or almost so, under the conditions which obtain during the formation of the salt, just as in the case of the conversion of potassium hydroxide into a salt; moreover, the product is easily soluble, even when acids like muriatic are used. Why the methyl-alcohol, or other methyl derivative, retained in the ammonium compound behaves so differently as compared with the unassociated methyl derivative, is a question which, for the present, we must be content to put aside unanswered. I am also of opinion that in discussing their constitution no particular weight can be attached to the mode in which the tetralkylic ammonium hydroxides undergo decomposition when heated, as the products in some cases are an amine and an alcohol, but in others an olefine and water, instead of an alcohol; in the case of the phosphonium salts the diversity is still greater (Chem. Soc. Proceedings, 1886, p. 164). That amines may act as "dehydrating" agents in the manner required if the molecular compound hypothesis be adopted, appears by no means improbable.

What is here stated of the tetramethyl compounds is true of tetralkylic ammonium haloid compounds generally, in the sense that they are all less readily acted on by alkalies than are the parent alkylic haloid compounds; but just as these latter are more readily attacked by alkalies and other agents the more complex the alkyl, so are the tetralkyl ammonium compounds; in no case, however, do they manifest a reactivity at all comparable with that of simple metallic or non-metallic haloid compounds—always excepting those of carbon.

The argument used above would apply equally to the phosphonium and sulphine compounds; indeed with greater force.

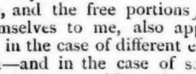
In many other respects the behaviour of nitrogen in aminic compounds is altogether peculiar and irreconcilable with the assumption of pentadicity. Thus it is commonly pointed out that the basic properties of aniline, for example, become lessened and ultimately almost annulled by the introduction of chlorine or bromine into the phenyl radicle; and that acetamide, $C_2H_3O . NH_2$, and other similar compounds formed by the introduction of acid radicles into ammonia are all but destitute of basic properties; the power to form ammonium compounds, therefore, is not a simple function of the nitrogen atom, but is largely dependent on the nature of the radicles associated with the nitrogen atom. Other illustrations are afforded by the hydrazines. Thus phenyl-hydrazine, $C_6H_5 . NH . NH_2$, although it contains two atoms of (triad) nitrogen, forms with hydrogen chloride the compound $C_6H_5 . N_2H_3 . HCl$, which crystallizes unchanged from fuming muriatic acid, in which, moreover, it is almost insoluble. Ethyl-hydrazine, however, forms a dichlorhydride, $C_2H_5 . N_2H_3 . 2HCl$, but on evaporating the aqueous solution of this salt a monochlorhydride is obtained; and unsymmetric diethyl-hydrazine, $(C_2H_5)_2N . NH_2$, is a monobase like phenyl hydrazine.

Hence it may well be argued that we have no reason to assume that nitrogen is pentad in the ammonium compounds, or phosphorus pentad in the phosphonium compounds, or sulphur tetrad [1] in the sulphine compounds; but that these are all to be reckoned as molecular compounds.

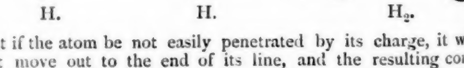
What then is the valency of the elements in question? and what is a molecular compound?

In answer to the first of these questions, the proposition may be advanced that gasefiable hydrogen compounds are the only compounds available for the direct determination of valency, and that the valency of an element—the number of unit charges necessarily associated with its atom—is given by the number of hydrogen atoms combined with the single atom of the element in its gasefiable hydride.[2] In cases where such hydrides are unknown, the determination of valency is very difficult; it can be but provisionally effected, and only by most carefully weighing all the evidence relating to the constitution of the compounds available for discussion.

But if it be granted, for example, that nitrogen is a triad, and that iodine is a monad, how are we to explain the fact that the methyl compounds of these two elements unite to form so well characterized a molecular compound as tetramethylammonium iodide? how are such molecular compounds constituted? My own view has long been that the nitrogen and iodine in such a case are both possessed of a certain amount of *residual affinity;* and I would define a molecular compound as one formed by the coalescence of two or more molecules, unattended by redistribution of the constituent radicles, and in which the integrant molecules are united by residual affinities. In other words, the unit charge must be capable in certain cases of directly promoting the association, not merely of two, but of at least three, atoms. To put this hypothesis in terms which cannot be misunderstood, let unit valency or charge be represented by a unit line, and further be it supposed that the charge *penetrates the atom*, then the atom with its unit charge may be represented thus:—



i.e. the unit charge may be held to consist of three portions, the buried portion a, and the free portions $f + f'$. The facts, as they present themselves to me, also appear to necessitate the assumption that, in the case of different elements, the charge penetrates the atom—and in the case of some polyad atoms, different directions in the atom—with varying degrees of freedom.[3] The union of two atoms may then be pictured as an overlapping of the unit lines. If the atoms are freely penetrated by their charges, each atom may tend to move out to the end of the line, leaving either no portion, or but a very small portion, free; a conception of this order would appear to apply in the case of hydrogen, and may be represented thus:—



H. H. H_2.

But if the atom be not easily penetrated by its charge, it will not move out to the end of its line, and the resulting compound molecule will possess more or less "residual affinity;" this conception would appear to apply to the non-metals generally, and to some of the metals; it may be illustrated thus:—

H. Cl. HCl.

I have thought it permissible to state my views in this form merely in order to advance the study of molecular compounds

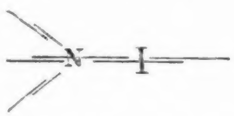
[1] Probably one of the strongest arguments in favour of the conclusion that sulphur is divalent may be based on its inactivity in the closed-chain compound *thiophen*, which does not unite with methyl iodide, nor does the sulphur in it or its homologues permit of oxidation in the manner that is characteristic of the element in thioethers.

[2] If this be granted, it follows that the maximum number of charges which an atom can carry is four; in other words, that the possible maximum valency is attained in the case of carbon.

[3] This is practically but a modification of Helmholtz's statement that "the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom, with different force."

by the introduction of a working hypothesis, an *absolutely artificial* mode of expression such as is here adopted being perhaps pardonable in the absence of any explanation which may serve to guide us in extending our inquiries as regards the structure of such compounds, a knowledge of which is all-important to a rational conception of the nature of chemical change generally. Moreover, I do not hesitate to affirm that, from the chemical point of view, it is impossible to adopt the Helmholtz explanation of valency, unless physicists are prepared to grant the possibility of the "division" of the unit charge somewhat in the manner here suggested; and it is in order to impress this that I have ventured to give utterance to these speculations.

To return to the consideration of the compounds previously referred to, it may be supposed that the nitrogen of trimethylamine and the iodine of methyl iodide are possessed of residual affinity, and hence the two molecules unite to form the molecular compound tetramethylammonium iodide, which may be represented thus:—



The phosphonium and sulphine iodides may be regarded as similarly constituted. It is well known that the ammonium haloid compounds and their analogues are also capable of forming still more complex molecular aggregates with the halogens, &c.: they are therefore to be regarded as possessed of residual affinity; and that polyad elements, *e.g.* nitrogen, phosphorus, and sulphur, should still exhibit residual affinity in such compounds is not surprising in the light of the hypothesis advocated in this note; but it is scarcely compatible with the assumption that the halogen in the ammonium haloid compounds serves as the bond of union. On the other hand, if it be assumed, as I think it should be, that the formation of double metallic chlorides, &c., is the outcome of the possession of residual affinity by the halogen, the complete analogy which appears to exist between the ammonium haloid compounds and those of the alkali metals would seem logically to involve the inference that the halogen of the ammonium compound does not serve as the bond of union. I see but one mode of escape from this conflict of evidence, and that is to call in question the time-honoured assumption that the radical ammonium is the true analogue of potassium and sodium, which, be it remarked, is of necessity subject to doubt if the hypothesis that the ammonium salts are molecular compounds be entertained; and evidence which supports the conclusion that the per-haloid compound is formed by the addition of the halogen to the nitrogen (phosphorus or sulphur) is afforded by the observation that not only *haloid* ammonium and sulphine compounds, but also the *sulphates*, combine with halogens (Dobbin and Masson, Chem. Soc. Trans., 1885, p. 56; 1886, p. 846).

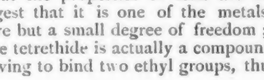
It is now proved by abundant experimental evidence that, whatever the order in which the radicles A, B, C, D are introduced in forming a tetralkylic ammonium compound N(ABCD)X, one and the same end product always results. This is commonly regarded as proof, not only that nitrogen is pentad, but also that the five affinities of the nitrogen atom are of equal value, and it would appear to favour the conclusion that the ammonium salts are in truth "atomic" compounds; but I see no reason why isomeric change should not occur at the moment of formation of a molecular compound—why the integrant molecules, in fact, should not interchange radicles. If the statement be confirmed[1] that the compound formed from dimethyl sulphide and ethyl iodide is different from that obtained on combining methylethyl sulphide and ethyl iodide (Krüger, *Journ. pr. Chem.*, 1876, xiv. p. 193), it will follow, not that sulphur is a tetrad, and that the four affinities are of unequal value, but that there is little or no tendency for isomeric change to occur in the formation of sulphines. The possible occurrence of isomeric change in the formation of molecular compounds, however, is a subject which certainly deserves careful study at the present time.

In the case of phosphorus, the existence of the highly stable gaseous pentafluoride PF_5, discovered by Thorpe, is undoubtedly regarded by many as final proof of the pentadicity of this element; but the existence of compounds such as H_2F_2, HFFR, &c., which clearly belong to the class of molecular compounds, is an indication of so marked a tendency on the part of fluorine to combine with itself, that for this reason alone (as Naumann and others have asserted) the pentafluoride is by no means necessarily regarded as an atomic compound. And I would here add that stability affords no criterion as between atomic and molecular compounds, every degree of stability being met with even among those of the former class.

An argument in favour of the pentadicity of phosphorus which apparently cannot be disposed of by any explanation based on conventional considerations has, however, been advanced by La Coste and Michaelis (*Berichte*, 1885, p. 2118), who have shown that the compounds obtained from diphenylchlorophosphine, $PCl(C_6H_5)_2$, and phenol is not identical with the triphenyl-phosphine oxide, $OP(C_6H_5)_3$, obtained by oxidizing triphenyl-phosphine, as it should be if the latter were a compound of the formula $(C_6H_5)_2P \cdot OC_6H_5$; this last corresponding to the formula $Cl_2P \cdot OCl$, which has been suggested as that of phosphorus oxychloride, and which appears to derive considerable support from Thorpe's observations on the specific volume of the oxychloride (Chem. Soc. Trans., 1880, p. 388). It is, however, conceivable that the oxygen and phosphorus are united by residual affinities, thus:—

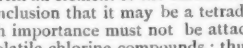
Michaelis and Polis (*Berichte*, 1887, p. 52) have argued in the case of bismuth, which also is a member of the nitrogen group, that the pentadicity of this element is proved by the existence of the triphenyl dibromide, $(C_6H_5)_3BiBr_2$. But the mere production of such a compound proves nothing so long as its constitution is undetermined; it at most serves to strengthen the conviction gained from the general study of the element, that bismuth is a member of the nitrogen-phosphorus group.

In other cases also it is possible that undue importance may have been attached to the existence of alkylic compounds of particular types: thus lead, judging from its general chemical behaviour, would appear to be a dyad; yet the existence of the tetrethi[de] $Pb(C_2H_5)_4$, is commonly held to be a proof that it can functi[on] as a tetrad. But the properties of lead are such that I am tempted to suggest that it is one of the metals in which the "charges" have but a small degree of freedom; and it is conceivable that the tetrethide is actually a compound of dyad lead, each charge serving to bind two ethyl groups, thus:—



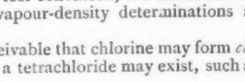
The same may be true of tin, although in this case the fact tha[t] we are dealing with an element of the carbon-silicon family tend[s] to favour the conclusion that it may be a tetrad.

Also too much importance must not be attached to the existence of stable volatile chlorine compounds: thus tellurium *tetra*chloride may well be a compound of dyad tellurium, thus:—



Iron, and the other members of the family which boron heads, in like manner, I feel convinced, are triads even in their *ic* compounds: recent vapour-density determinations all support this conclusion.

It is even conceivable that chlorine may form *closed-chain* compounds, and that a tetrachloride may exist, such as is represented by the formula:—



I think it is especially noteworthy that so many well characterized and comparatively stable double chlorides exist formed by the union of chlorides of which one at least is *per se* very unstable; the tin-sulphur chloride, $SnCl_4 \cdot 2SCl_4$, and the remarkable series of aurous compounds recently described by Lepetit (*Ann. Chim. Phys.*, 1887, p. 11) may be cited as examples.

[1] The number of *Liebig's Annalen* last issued contains a valuable paper by Klinger and Maassen disproving Krüger's statement.

If my contention in this and previous papers be correct, that residual affinity thus plays a far more important part than has hitherto been supposed, and that it must be taken into account in all discussions on valency, it follows of necessity that our views regarding the constitution of the majority of compounds at present rest upon a most uncertain basis: the constitution of the paraffins, of the benzenes, and of the haloid compounds and alcohols derived from the hydrocarbons of these series, may be regarded as determined with a degree of precision almost amounting to certainty; but in the vast majority of other cases we have as yet no secure method of arriving at conclusions which in any sense approach finality. There can be little doubt that in framing our modern conceptions of valency we have been too much influenced by the graphic symbols which have been so widely made use of. In the future it will be necessary to attach a more liberal interpretation to the facts, and it may be hoped that it will some day be possible also to take into account differences depending on the relation of the different forms of matter to the pervading medium.

The properties of compounds being demonstrably dependent on the intramolecular conditions, it is difficult for a chemist to resist the feeling that the peculiarities manifested by the different elements are also very probably the outcome of differences in structure; such an assumption indeed affords at present apparently the only explanation that can be given of the relationship manifest between different elements when these are classified in groups of "homologues" in accordance with the suggestion originally made by Dumas, which has now found full expression in the so-called periodic system of classification. There appears to be an increasing weight of evidence to favour the assumption that the influence exercised by compounds in cases of chemical change is local in its origin: that it is exercised more by a particular constituent or constituents—in particular directions, in fact—than by the molecule as a whole. The suggestion above made that "affinity" acts in particular directions in elementary atoms, and perhaps with different degrees of freedom in various directions, is therefore but an extension to elements of what is more or less generally recognized as the case in compounds. Some such hypothesis is certainly required to account for the existence of allotropic modifications both of non-metals and of metals; for the remarkable changes in magnetic and other properties which iron undergoes with change of temperature; for the different values of the dielectric constant—along the several axes in sulphur crystals; for the difference in electric conductivity of bismuth in two different directions in bismuth crystals; for the existence of planes in crystals in which cleavage takes place with special readiness, &c.—all these are instances which apparently afford evidence of atomic dissymmetry. May not valency after all depend—not in the number of "charges" carried by the atom, but—on the number of directions in which the ever-present "lines of force" are free to act?

WORK OF THE KEW OBSERVATORY IN 1887.

THE Annual Report of the Kew Committee, just issued, shows that the activity of the staff of the Kew Observatory is still well sustained, and the various departments devoted to observations—magnetic, meteorological, and solar—verification of scientific apparatus of various kinds, rating of time-pieces, and experiment, all show a considerable turn out of work. In addition to the regular periodical magnetical observations, the main results of which are given in a concise form in the appendixes, assistance was rendered to Profs. Rücker and Thorpe in respect to their valuable magnetic survey of Great Britain, which we are glad to learn they have now completed, after having devoted the greater portion of their vacations to the task for the last four years. The labours of the Krakatão Committee of the Royal Society, the Magnetic Committee of the British Association, the late Prof. Balfour Stewart, and other investigators, have also been supplemented by aid afforded by the Kew staff.

The meteorological staff have during the year recorded, principally on behalf of the Meteorological Council, who defray the expenses attendant on the work, some 57,126 observations averaging over 150 per diem; the resulting monthly and annual means are, by permission of the Council, published as appendixes.

The multiplication of Observatories engaged in solar photography at home and abroad having rendered unnecessary the co-operation of Kew in that branch of science, so energetically carried on there by the present Chairman, Mr. De la Rue, and the late Prof. Balfour Stewart, twenty years ago, the photoheliograph has only been employed of late years as an ordinary telescope, by means of which the counting of new sunspot groups is continued after Schwabe's method.

An appendix shows that during the last year 44 new groups were catalogued, and that on 60 days out of 180 days of observations the sun's surface was free from spots.

Under the heading "Experimental Work" we find a good deal of attention has been devoted to the photography of high cirrus clouds simultaneously from two points, with the view of determining their position and motions; and to the question of the proper construction of black bulb thermometers; and also to preparatory operations with the Indian Government pendulum apparatus, preliminary to repeating the observations made at the Observatory by Basevi, Heaviside, and Herschel.

There is a long list of various instruments compared and certified during the year in the verification department, which shows that nearly 14,000 articles belonging to one or the other of twenty-seven different classes have undergone treatment; as instruments newly brought within the influence of the verifier, attention is directed to range-finders for the use of the Army and Navy, telescopes of the Admiralty pattern, and surveying aneroids.

The popularity of the Kew certificates, as to the time-keeping of watches, shows that the demand for a guarantee as to the accuracy of performance of a watch other than the maker's name actually exists, and no less than 510 watches and 27 marine chronometers have been submitted to the rating department since the last report was issued.

An appendix showing the behaviour of the best of the watches during the test is given, and it is found that places in this list are being strongly contested for by watch manufacturers, as the blue ribbons of the trade. In consequence of the growth of the work done at Kew, steps have been taken to obtain the permission of Her Majesty's Chief Commissioner of Works and Public Buildings to enlarge the Observatory, which at present remains almost in the same condition as it stood 130 years ago when originally erected as His Majesty George III.'s private Observatory at Richmond.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.

BY the kindness of the Astronomer Royal for Scotland, Mr. Gledhill, of Mr. Crossley's observatory at Bermerside, and Mr. Stothert, all of whom took part in the observation of the eclipse of October 4, 1884, we are enabled to give Prof. Struve's times and position-angles for the stars that will be seen to be occulted by observers stationed at Edinburgh, Halifax, and Bath. A comparison of these tables will enable intending observers in other parts of England to form a sufficiently correct list for their own locality.

The following ten stars, not included in the list given in NATURE for January 19, will be occulted as seen from Edinburgh:—

Star's No.	R.A. ° ′	Decl. ° ′	Star's No.	R.A. ° ′	Decl. ° ′
103...	130 30·76...	17 18·71 N.	183...	131 13·94...	17 8·64 N.
106...	33·62...	16·68	206...	24·79...	7·44
117...	37·99...	14·54	213...	29·29...	6·11
129...	44·71...	13·74	217...	31·16...	5·36
141...	53·76...	14·84	228...	34·65 .	6·26

Star No. 106 is of mag. 9·3; No. 129, 9·5; No. 206 is of the 10th magnitude; the others are all of the 11th magnitude.

EDINBURGH.

Lat. = 55° 57′ 23″; Long. = 3° 10′ 54″ W.

Disappearances.				*Reappearances.*			
Star's No.	Mag.	Angle. °	G.M.T. h. m.	Star's No.	Mag.	Angle. °	G.M.T. h. m.
152 ...	11 ...	94 ...	10 24·3	108 ...	9·3 ...	333 ...	10 26·0
150 ...	10 ...	116 ...	26·9	87 ...	11 ...	259 ...	26·0
142 ...	10 ...	135 ...	27·2	103 ...	11 ...	226 ...	28·5
148 ...	10 ...	57 ...	27·8	106 ...	9·3 ..	216 ...	29·8
129 ...	9·5 ...	173 ...	28·0	91 ...	11 ...	290 ...	30·3

EDINBURGH—(*continued*).

Disappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
153	10	114	10	29·6
Beginning of total phase				
156	11	64	10	33·4
141	11	152		35·0
164	8·0	97		37·3
165	9·4	101		37·9
166	9·5	74		39·6
157	9·4	45		40·5
155	11	141		42·3
172	11	128		50·1
180	9·5	70		52·9
181	10	43	11	7·8
198	9·5	87		15·2
197	9	114		17·6
190	11	143		19·7
207	11	85		23·1
209	10	97		24·9
194	11	32		27·5
210	9·5	70		27·5
183	11	171		30·1
212	11	114		33·9
201	8·7	31		36·4
216	10	111		38·2
223	11	80		40·3
225	10	94		42·2
224	11	53		48·3
226	10	124		48·5
206	10	163		49·2
219	10	147		55·3
236	9·5	94		55·5
221	10	30		57·6
213	11	166	12	0·9
233	11	140		4·2
237	11	54		4·4
242	11	105		5·0
217	11	168		6·6
228	11	157		7·2
End of total phase				
247	9·2	75	12	16·1

Reappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
117	11	200	10	30·3
98	11	300		30·9
Beginning of total phase				
100	9·5	302	10	31·3
93	11	292		31·4
102	11	250		35·1
114	11	235		46·4
129	9·5	206		48·1
110	11	277		49·2
125	11	235		53·5
134	11	334		55·0
126	9·5	282	11	1·0
128	9·5	295		2·2
141	11	227		19·0
138	11	272		20·4
148	10	323		20·4
157	9·4	335		22·1
144	11	307		22·7
142	10	245		26·3
156	11	318		31·3
152	11	287		35·9
150	10	265		36·8
155	11	240		37·2
166	9·5	306		44·7
181	10	340		45·2
164	8·0	284		49·5
165	9·4	280		50·4
194	11	351		53·3
172	11	254		54·7
180	9·5	311		
183	11	213	11	56·2
201	8·7	352	12	0·9
End of total phase				
190	11	240	12	14·3

BERMERSIDE, HALIFAX.

Lat. = 53° 42′ 10″; Long. = 1° 5′ 58″ W.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
136	9·5	29	10	23·9
152	11	101		24·3
148	10	67		24·4
150	10	123		28·9
142	10	144		30·9
156	11	72		31·0
Beginning of total phase				
153	10	122	10	31·6
157	9·4	57		36·0
164	8	105		37·8
166	9·5	82		38·3
165	9·4	108		38·8
141	11	167		42·7
155	11	152		47·4
180	9·5	79		51·7
172	11	137		53·5
181	10	53	11	3·3
198	9·5	93		15·6
197	10	121		20·4
194	11	47		21·0
207	11	91		23·6
190	11	153		25·7
209	10	104		26·4
210	9·5	78		26·9
201	8·7	47		30·0
212	11	121		36·8
223	11	88		40·8
216	10	118		41·0
225	10	100		43·8
224	11	62		46·2
221	10	46		51·3
226	10	137		53·5
236	9·5	99		57·5
206	10	181	12	2·2

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
103	11	212	10	20·8
87	11	252		23·9
112	11	342		25·1
115	11	342		27·1
108	9·3	323		30·9
91	11	283		30·9
Beginning of total phase				
93	11	185	10	32·1
98	11	293		32·4
102	11	242		32·4
100	9·5	295		32·9
114	11	225		41·8
130	11	339		45·7
136	9·5	352		47·4
125	11	225		49·0
110	11	271		49·3
134	11	325	11	0·7
126	9·5	276		2·1
128	9·5	289		4·0
141	11	215		13·1
138	11	267		21·0
142	10	238		24·3
148	10	315		25·2
144	11	301		25·9
157	9·4	326		28·3
155	11	232		34·5
156	11	310		35·5
150	10	258		37·1
152	11	281		37·9
153	10	261		40·5
166	9·5	301		48·3
164	8	278		51·6
165	9·4	274		52·1
181	10	331		52·5

BERMERSIDE—(*continued*).

Disappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
219	10	157	12	2·4
237	11	63		3·0
242	11	110		7·8
End of total phase				
233	11	147	12	9·9
213	11	187		16·1

Reappearances.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
172	11	247	11	54·4
180	9·5	305	12	0·0
194	11	338		3·0
End of total phase				
201	8·7	339	12	11·1
190	11	232		12·9

BATH.

Lat. = 51° 23′ 19″; Long. = 2° 22′ 51″ W.

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
152	11	108	10	22·6
156	11	80		27·0
150	10	131		29·3
157	9·4	66		30·3
Beginning of total phase				
153	10	129	10	31·7
142	10	156		34·3
166	9·5	90		35·3
164	8·0	112		36·6
165	9·4	116		37·9
180	9·5	87		48·5
155	11	165		52·9
172	11	146		55·9
181	10	64		57·5
198	9·5	102	11	13·9
194	11	58		14·5
197	10	128		21·2
207	11	98		21·9
201	8·7	58		23·4
210	9·5	86		24·3
209	10	111		25·9
190	11	167		32·0
212	11	128		38·0
223	11	95		39·2
216	10	126		42·0
224	11	71		42·2
225	10	108		43·5
221	10	57		45·1
226	10	139		55·2
236	9·5	107		57·4
237	11	72		59·5
242	11	117	12	8·6
End of total phase				
219	10	172	12	10·7
233	11	158		15·2
247	9·2	88		16·0

Star's No.	Mag.	Angle. °	G.M.T. h.	m.
97	11	316	10	20·5
102	11	233		26·6
124	11	349		27·0
116	11	338		27·3
91	11	276		29·1
112	11	329		29·7
93	11	278		30·4
114	11	208		30·9
Beginning of total phase				
98	11	286	10	31·3
115	11	329		31·7
100	9·5	288		32·2
108	9·3	314		32·6
125	11	208		38·3
110	11	264		46·6
130	11	328		50·2
136	9·5	336		55·2
126	9·5	268	11	0·1
128	9·5	282		3·2
134	11	316		3·3
142	10	226		17·8
138	11	259		18·3
144	11	294		26·3
155	11	218		26·5
148	10	307		26·9
157	9·4	316		31·4
150	10	251		34·0
152	11	274		36·9
156	11	302		36·9
153	10	253		37·8
166	9·5	292		49·1
172	11	238		50·2
164	8·0	271		50·6
165	9·4	267		50·9
181	10	321		56·8
180	9·5	297	12	1·6
190	11	219		5·8
194	11	327		8·6
End of total phase				
201	8·7	328	12	17·1

The Chief Assistant, Royal Observatory, Greenwich, Mr. H. H. Turner, will be obliged if successful observers will write or telegraph to him, *immediately* after the eclipse is over, the number of immersions and emersions observed, and the character of the night for observing.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Prof. Sylvester is not to lecture this term; Mr. Es on will give for him courses on Higher Plane Curves and on Involution.

Prof. Pritchard promises two lectures on Modern Methods of determining Stellar Parallax, besides a longer course on Lunar and Planetary Theories.

In Physics, Prof. Clifton is giving only an elementary lecture; Mr. Walker lectures on the Polarization of Light, treated mathematically, and Mr. Baynes is to take up Fourier's Theorem and its Application to the Conduction of Heat.

In Chemistry, besides the usual systematic courses, Prof. Odling is lecturing on the Paraffins, and Mr. Veley on Physical Chemistry.

In the absence of Prof. Moseley, Dr. Hickson is lecturing on the Morphology of the Chordata; Mr. Hatchett Jackson lectures on Parthenogenesis.

Prof. Burdon Sanderson is treating of the Nervous System, and Prof. Bayley Balfour of the Algæ.

Dr. Tylor is giving Anthropological Elucidations of Greek and Latin authors, and the Reader in Geography is continuing the courses which he began last term.

The Professorship of Geology is to be filled up in the course of this term: applications will be received by the Registrar up to February 1.

The Radcliffe Fellowship will be awarded this term; also the Burdett-Coutts Scholarship, as soon as the Professor of Geology is appointed.

The number of men reading Medicine is steadily increasing, and now that it is possible for a man to pass his B.A. examinations and his first M.B. examination in four years, there can be little doubt that the increase will continue.

CAMBRIDGE.—Mr. H. D. Rolleston, B.A., M.B., of St. John's College, has been appointed Demonstrator of Pathology.

Sir F. A. Abel, F.R.S., has been appointed to deliver the Rede Lecture this year.

The Disney Professor (the Rev. G. F. Browne) will lecture on Tuesdays this term on Sculptured Stones of pre-Norman type in the British Islands. The inaugural lecture will be given in the Senate House on January 31.

Dr. F. Warner's lectures on the Growth and Development of the Intellectual Faculty began on January 25. The lectures aim at describing and analyzing the action of the brain of a child, with special application to educational methods.

SCIENTIFIC SERIALS.

THE most important article in the numbers of the *Journal of Botany* for December 1887 and January 1888 is one by Mr. S. Le M. Moore, "On Epidermal Chlorophyll," in which he shows that the presence of chlorophyll-corpuscles in the cells of the epidermis is of much more common occurrence than is usually stated in text-books, and that these chlorophyll-corpuscles very commonly contain starch-grains. The other articles relate to botanical nomenclature and to the distribution of British plants. In addition, Mr. J. G. Baker continues his "Monograph of the Tillandsieæ," and Colonel Beddome contributes a paper on Ferns of Perak and Penang.

IN the *Botanical Gazette* (Crawfordsville, Indiana) for December 1887 is an interesting paper by Mr. Byron D. Halsted, showing that under certain conditions pollen-grains may contain three nuclei, instead of the two usually found in them.

Bulletin de l'Académie Royale de Belgique, November 1887.—Action of the acids on the taste, by J. Corin. The object of these researches has been to ascertain what relation there may exist between the acid taste and chemical composition. The author arrives at the curious result that acidity increases with the quantity of basic hydrogen contained in the acid molecule, and decreases with the weight of the molecule itself.—Physical observations on Saturn, by Paul Stroobant. These observations, extending over the period from January 27 to April 20, 1887, show that the famous divisions of Encke and Struve appear to be subject to great modifications, especially as regards the actual position occupied by them. No doubt the state of the terrestrial atmosphere, the proximity of the moon, and other outward circumstances, must exercise a considerable influence on the character of the manifestations. But the changes here recorded, such as the disappearance of Encke's division while that of Struve is still visible, make it evident that other and more recondite causes are at work in producing these shifting appearances.—Experimental researches on the sense of vision in the Arthropods (second part), by Felix Plateau. In this section the author passes from a study of the Myriapods to that of the higher order of the Arachnidæ, and arrives at the general conclusion that in all the sub-groups of the Spiders, Scorpions, and Phalangidæ the visual sense is very feebly developed. They exhibit in general a vague perception of movement acting on their nervous system, rather than a clear sight of any definite object. In the case of ***Epiblemum scenicum***, distinct vision does not seem to extend beyond a distance of 1 centimetre, while *Tegenaria domestica* and others seem unable to distinguish form at all. Even in the closest proximity they rush with equal avidity on true or false objects of prey. The scorpions also show little evidence of sight, shunning the light and awaiting, rather than pursuing, their prey, which they fail to detect except at very short distances. The same remark applies to the Phalangidæ, which compensate the defect of vision by the exquisite tactile sense of their extremities.

Rivista Scientifico-Industriale, November 1887.—On the heating of metallic points when discharging their electricity, by Prof. Eugenio Semmola. Some experiments are described scientifically demonstrating the fact that heat is generated while metallic points discharge their electricity, the points themselves becoming at the same time heated. It is suggested that this fact, now for the first time verified, might under certain conditions be utilized as a new means of studying atmospheric electricity.—On the anæsthesis and poisoning of plants, by Dr. Flaminio Tassi. An analysis is given of the researches and experiments carried out by Prof. T. Caruel, tending to show that certain plants really possess a property analogous to the irritability, excitability, sensitiveness, or contractibility of animals, as it is variously called; that this property is not derived from any particular nervous system, but from the vegetable protoplasm itself; that certain organic substances are alike fatal to plants and animals; and that a state resembling animal anæsthesis is also produced especially in those plants which are endowed with excitable organs, and in many flowers that open and close at fixed times.

THE last two parts of vol. xviii. of the *Izvestia* of the East Siberian branch of the Russian Geographical Society contain a variety of valuable information. In a paper on the lower course of the Upper Angara, Dr. Kiriloff brings together some interesting facts about the fishing in Lake Baikal, which, notwithstanding complaints about the disappearance of the *Salmo omul*, still yields every year about 30,000 cwt. of fish. MM. Priklonsky and Slyeptsoff contribute notes on the religious beliefs of the Yakutes, who, although christened, have retained in full their Shamanist religion and practices. M. Karpinsky gives some notes on the gold-diggings of the Olekma system. Especially valuable papers are contributed by M. Savenkoff, on his archæological researches on the Yenisei, and by M. Eleneff on the caves on the banks of the Biryusa River. It would be impossible to enumerate in a short note all the interesting data mentioned in M. Savenkoff's preliminary report. His numerous collections contain, among other things, big bones of the mammoth and the rhinoceros, which bear unmistakable traces of having been broken by man for the sake of the marrow, and thus belong to the very rare relics of the Palæolithic period in Siberia. His collections also include bones with grooves for the insertion of a stone arrow-head, and many interesting implements, showing that stone implements were largely used during the Bronze Age, and partly during the Iron Age. The full report of M. Savenkoff, which will contain accurate drawings of the Yenisei inscriptions, will be most valuable. As to the exploration of caverns on the banks of the Biryusa and the Yenisei, M. Eleneff gives only a short description of his diggings, with detailed drawings and lists of the implements and various things found: Chinese money from the thirteenth or fourteenth century in the upper layers, various iron implements in the middle layers, and Neolithic stone implements in the lowest layers. The same parts of the *Izvestia* contain preliminary reports about an excursion to Lake Kosogol and the Munku-Sardyk, by MM. Prein and Yaczewski, during which excursion the glacier of this peak was thoroughly mapped and photographed, and large collections of Alpine flora were gathered.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 22, 1887.—"Heat Dilatation of Metals from Low Temperatures." By Thos. Andrews, F.R.S.E.

The experiments of this paper were made to approximately determine the coefficients of heat dilatation of modern steels from low temperatures. The metals employed were wrought iron, "soft" Bessemer steel, "hard" Bessemer steel, "soft" Siemens-Martin steel, "hard" Siemens-Martin steel, "soft" cast steel, "hard" cast steel, &c., of known composition, specific gravity, &c., given in detail in the paper. The terms "soft" and "hard" relate only to difference of percentage of combined carbon. The ranges of tempera-

ture chosen for the observations were from − 45° C. to 300° C. The experiments were made on rolled bars of the various steels and also on large hammered forgings 5 inches diameter. Details are given in the paper of the general method of experimentation, and also of the methods adopted for reducing the metals to the very low temperature employed. The results of an extensive series of experiments are recorded in tabular form in the paper. The coefficients of dilatation were found generally to decrease with the reduced temperature. The author also found such to be the case in his recent observations on the heat dilatation of pure ice from low temperatures. There seemed to be a slightly greater dilatation in the direction of the length of the forged metallic cylinders than when measured across the diameter. It was also noticed that the coefficients of dilatation were greater in the case of steels having a lower percentage of combined carbon than in those containing a higher percentage.

January 12.—"Invariants, Covariants, and Quotient Derivatives associated with Linear Differential Equations." By A. R. Forsyth, F.R.S.

The memoir deals with the covariantive forms associated with the general ordinary linear differential equation. The most general transformation to which such an equation can be subjected without changing its character is one whereby the dependent variable y is changed to u by a relation $y = u f(x)$, and at the same time the independent variable is changed, say, from x to z. When these transformations are effected there are n relations between the coefficients P and Q of the equation in its two forms, and it is shown that from these others can be deduced which are of the form

$$\psi(\mathrm{P}) = \left(\frac{dz}{dx}\right)^{\rho} \psi(\mathrm{Q}).$$

Such a function ψ is called an invariant of index ρ.

Irreducible invariants are proved to be divisible into two classes, fundamental and derived. Each of the former, which are $n - 2$ in number, consists of two parts; one of these is linear in the quantities P and their derivatives, the other is not linear, but has in every term as a factor either P_2 or some derivative of P_2. It is shown that the differential equation can be reduced to a canonical form without any term in $\frac{d^{n-1}u}{dz^{n-1}}$ or $\frac{d^{n-2}u}{dz^{n-2}}$; and hence each of the prior class of invariants is linear in the coefficients of the canonical form and their derivatives. These fundamental invariants are called priminvariants. The derived invariants are obtained from the priminvariants by two processes, which are called the quadriderivative and the Jacobian; they are most conveniently arranged in classes according to their degrees in the coefficients of the equation. The number of quadrinvariants is $2n - 5$; the number of invariants of every degree higher than the second is $n - 2$.

The relation between the independent variables of a semi-canonical form and of the canonical form shows that the dependent variable may be considered as a covariant. It is proved that there are other $n - 2$ associate dependent variables, each satisfying a linear equation and possessing for the canonical form the invariantive property.

From this aggregate of dependent variables, a set of irreducible identical covariants is derived by the two processes formerly used for the invariants; when the equation is taken in its canonical form, all these covariants up to a certain order involve the dependent variables alone. There is also a set of irreducible mixed covariants which are the Jacobians of each of the dependent variables in turn, and one of the invariants.

Illustrations of the results are given for the equations of the second, the third, and the fourth orders; and in this connection, functions, called quotient derivatives, are obtained. Some of their properties are given, one of the most important being that they are covariantive for homographic transformation of both the dependent and the independent variables.

Finally, the characteristic differential equations satisfied by all concomitants are obtained; and among other inferences it is proved that the aggregate of concomitants constituted by the invariants and covariants obtained in the earlier part of the memoir is complete, *i.e.* that any concomitant can be algebraically expressed in terms of the members of that aggregate.

"Preliminary Note on the Nephridia of Perichæta." By Frank E. Beddard, M.A.

The following observations are the result of a study of a species of Perichæta, which is probably identical with Perrier's *P. aspergillum*. I owe a number of excellently preserved examples to the kindness of Mr. Shipley, Fellow of Christ's College, Cambridge.

In transverse sections of the anterior segments the nephridia are seen to form numerous tufts of glandular tubules closely related to the body-wall and to the septa. This appearance, which is also seen in dissections, is very different from that of most earthworms, and has been commented upon by other observers.

The remarkable appearance of the nephridia led me to infer that I should find the external apertures in each segment to be numerous, as I showed to be the case in Acanthodrilus. I am now able to state that this is also the case in Perichæta (in all probability in other species besides *P. aspergillum*). The external pores lie between the setæ, but have no regularity in their arrangement; frequently there were three or four between two successive setæ, as often there seemed to be only one or two. The minute structure of the terminal section of nephridia is slightly different from that of Acanthodrilus. Another point, to which I wish to direct attention in this communication, is that *in Perichæta there is a connection between the nephridia of successive segments.*

Quite recently, Ed. Meyer and Cunningham have shown that in *Lanice conchilega* the nephridia of each side are connected by a continuous longitudinal duct. This discovery is in accord with the presumed origin of the Annelid from the Platyhelminth excretory system, and also with the development of Polygordius (Hatchek) and Lumbricus. In Perichæta the connection between the nephridial tufts of successive segments is not brought about by a continuous longitudinal duct, one on each side of the body, but by numerous tubules which perforate the intersegmental septa. Thus it appears that *the nephridial system of Perichæta consists of a network of tubules.* In this respect Perichæta agrees with the leech Pontobdella, but differs in the presence of numerous nephridiopores in each segment. These facts appear to lend further support to the view that it is possible to derive the Annelid from the Platyhelminth excretory system.

Lang has pointed out that the "secondary" pores by which the excretory organ of the Platyhelminths communicates with the exterior have probably given rise to the nephridial pores in the Annelida; by a subsequent arrangement of these in a metameric fashion, and by the breaking up of the nephridial network, the paired nephridia have originated. The longitudinal canal has disappeared, except in the cases that I have already mentioned. In some Platyhelminths the longitudinal canals are, partly at least, broken up into a network; and it is this condition which has persisted in Perichæta and Pontobdella; moreover, in some Platyhelminths, where the "secondary" pores have become metamerically arranged, there are more than one pair to each "segment." For this reason it is perhaps allowable to regard the condition of the nephridia in Perichæta as more archaic than Pontobdella. The disappearance of the connection between the nephridia of successive segments leads to the condition which exists in Acanthodrilus; the reduction of the external pores, already perceptible in the posterior segments of *A. multiporus*, culminates in the disappearance of all but two in each segment. The irregularity in the position of these, which is best marked in Plutellus, is the last trace of the presence of multiple nephridiopores in each segment.

Royal Meteorological Society, January 18.—Mr. W. Ellis, President, in the chair.—The paper read was on the non-instrumental meteorology of England, Wales, and Ireland, by Mr. G. M. Whipple. This is a discussion of the observations of wind, cloud, thunderstorms, hail, snow, &c., made at the stations of the Royal Meteorological Society during the eight years 1878–85, and published in the *Meteorological Record.* The S.W. wind is the most prevalent, and blows on the average seventy-four days in the year; the W. wind occurs almost as frequently, blowing sixty-five days. The least dominant winds are the S.E. and N., which occur on twenty-seven days, and the N.E. on thirty-two days. Thunderstorms are most frequent in the eastern and midland counties, and least frequent in the north of Wales.—After the reading of this paper, the annual general meeting was held. The report of the Council showed the Society to be in a satisfactory condition, the number of Fellows

being 522.—Mr. Ellis in his Presidential address reviewed briefly the work and position of the Society, remarking that such a Society, whilst unable to carry out expensive original or experimental work, could yet act with great advantage in inciting volunteer workers throughout the country to united action, of which one recent example was the ready response to the request of the Society for photographs of lightning, an excellent collection of which had been obtained, and which would shortly be exhibited; in addition to which arrangements were being made for the more systematic observation of thunderstorms. Referring to the question of sympathetic relation between sunspots and magnetism and meteorology, he thought that any complete treatment of the question in its meteorological aspect seemed to require that it should be dealt with in a much more comprehensive manner than before, for which purpose observations more completely covering the surface of the globe might be necessary, if indeed not necessary also for the solution of many other meteorological questions, the present meteorological stations being distributed over the earth in such isolated clusters. The attention given to synoptic charts was most important, but the general meteorological characteristics of places should also still continue to be studied. After remarking upon other matters, he laid before the meeting tables showing the monthly means of amount of cloud from observations made in three different series at the Royal Observatory, Greenwich, extending in all from 1818 to the present time. In concluding, Mr. Ellis said that at one time the science of meteorology seemed likely to form an exception to the general rule of advance, for more than any other it has required the united action of many workers, but the field of inquiry of late years opened out allows us already to talk of the new or modern meteorology, phrases typical of the advance achieved, although the knowledge gained seems only to remind us of how much has yet to be done.—The following gentlemen were elected the officers and Council for the ensuing year:—President: Dr. Wm. Marcet, F.R.S. Vice-Presidents: Francis Campbell Bayard, William Ellis, Charles Harding, Richard Inwards. Treasurer: Henry Perigal. Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver. Secretaries: George James Symons, F.R.S., Dr. John William Tripe. Foreign Secretary: Robert Henry Scott, F.R.S. Council: Hon. Ralph Abercromby, Robert Andrew Allison, M.P., Edmund Douglas Archibald, William Morris Beaufort, Henry Francis Blanford, F.R.S., Arthur Brewin, George Chatterton, William Henry Dines, Henry Storks Eaton, Baldwin Latham, Edward Mawley, Dr. Charles Theodore Williams.

Chemical Society, December 15, 1887.—Mr. William Crookes, President, in the chair.—The following papers were read:—An apparatus for comparison of colour-tints, by Alfred W. Stokes.—The alloys of copper and antimony and of copper and tin, by E. J. Ball.—The constitution of the so-called mixed azo-compounds, by Francis R. Japp, F.R.S., and Felix Klingemann.—The interpretation of absorption-spectra, by G. H. Bailey.—The reduction of potassium bichromate by oxalic acid, by C. H. Bothamley.—The reduction of chlorates by the zinc-copper couple, by C. H. Bothamley and G. R. Thompson.—Preliminary notice on the oxidation of oxalic acid by potassium dichromate, by Emil A. Werner.—Isomeric change in the naphthalene series; No. 1, by Henry E. Armstrong.—Isomeric change in the naphthalene series; No. 2, β-Ethoxynaphthalenesulphonic acids, by E. G. Amphlett and Henry E. Armstrong.—Isomeric change in the naphthalene series; No. 3, β-Chloronaphthalenesulphonic acids, by Henry E. Armstrong and W. P. Wynne.—Isomeric change in the naphthalene series; No. 4, α-Haloidnaphthalenesulphonic acids, by Henry E. Armstrong and S. Williamson.—The sulphonation of naphthalene, by Henry E. Armstrong and W. P. Wynne.

Entomological Society, January 18.—Fifty-fifth aniversary meeting.—Dr. D. Sharp, President, in the chair.—An abstract of the treasurer's accounts was read by Mr. H. T. Stainton, F.R.S., one of the auditors; and Mr. H. Goss, the Secretary, read the Report of the Council.—It was announced that the following gentlemen had been elected as Officers and Council for 1888:—President: Dr. David Sharp. Treasurer: Mr. Edward Saunders. Secretaries: Mr. Herbert Goss and the Rev. Canon Fowler. Librarian: Mr. F. Grut. As other Members of Council: Mr. Henry J. Elwes; Sir John Lubbock, Bart., M.P., F.R.S.; Mr. Robert McLachlan, F.R.S.; Dr. P. Brooke-Mason; Mr. Edward B. Poulton; Mr. Osbert Salvin, F.R.S.; Mr. Henry T. Stainton, F.R.S.; and Lord Walsingham, F.R.S.—The President delivered an address, and a vote of thanks to him was moved by Mr. McLachlan, seconded by Mr. F. Pascoe, and carried.—A vote of thanks to the Treasurer, Secretaries, and Librarian, was moved by Mr. Kirby, seconded by Mr. Waterhouse, and carried. Mr. E. Saunders, Mr. H. Goss, Canon Fowler, and Mr. F. Grut replied.

Mathematical Society, January 12.—Sir J. Cockle, F.R.S., President, in the chair.—Messrs. J. M. Dodds and G. G. Morrice were elected members, and Mr. E. W. Hobson admitted into the Society.—The following communications were made:—The theory of distributions, Capt. P. A. Macmahon, R.A.—On the analogues of the nine-points circle in space of three dimensions, S. Roberts, F.R.S.—On a theorem analogous to Gauss's in continued fractions with applications to elliptic functions, L. J. Rogers.—A theorem connecting the divisors of a certain series of numbers, Dr. Glaisher, F.R.S.—On reciprocal theorems in dynamics, Prof. H. Lamb, F.R.S.

Mineralogical Society, January 10.—Mr. L. Fletcher, President, in the chair.—The following papers were read:—On the development of lamellar structure in quartz crystals by mechanical means, by Prof. J. W. Judd, F.R.S.—On the polysynthetic structure of some porphyritic quartz crystals in a quartz-felsite, by Colonel C. A. McMahon.—Notes on hornblende as a rock-forming mineral, by Mr. A. Harker.—On the invitation of the President, Mr. Allan Dick, who was present as a visitor, made some remarks on the process of kaolinization, illustrated by models of crystals.

PARIS.

Academy of Sciences, January 16.—M. Janssen, President, in the chair.—Remarks on M. Wolf's last note on the subject of synchronization, by M. A. Cornu. The author is glad to find himself in harmony with M. Wolf on the important points that no synchronizing system is possible without some controlling or regulating apparatus, and that such apparatus forms an essential feature of the systems of Jones and Vérité.—Remarks accompanying the presentation of the third volume of the "Annales de l'Observatoire de Rio Janeiro," by M. H. Faye. This volume, which was presented by the Emperor of Brazil, is entirely devoted to the three Brazilian expeditions sent to the Island of Saint Thomas, Pernambuco, and Punta Arenas (Patagonia) to observe the transit of Venus in the year 1882. From a comparative study of the recorded results, M. Cruls has calculated the solar parallax at 8″·808.—Fresh researches on the phenomena produced by a potent toxic agent, which is constantly emitted with the air exhaled from the lungs of man and other mammals, by MM. Brown-Séquard and d'Arsonval. The experiments here described and made on seven rabbits entirely confirm the conclusions already announced regarding the powerful character of this volatile organic poison, which appears to be almost certainly an alkaloid. Further researches have been undertaken in order to determine this point by direct proof.—On spontaneous tetanus, by M. Verneuil. A case reported by Dr. Buisson, of Aubercicourt, is referred to as confirming in a striking way the author's opinion that there is no such thing as spontaneous tetanus, and that all reported cases will be found, if carefully studied, to be caused by some virus introduced in some way into the system.—On the canalization of the Isthmus of Panama, by M. de Lesseps. In supplement to his recent remarks on this scheme the author announced that the proposal to establish provisionally a lock canal for one at a dead level has just been adopted by the Company. He further explained how the extensive works already executed can be adapted to the new design, so that the Canal might still be completed and opened for traffic by the year 1890. It would moreover be so constructed that the original plan of a level canal might be gradually carried out without any interruption to the navigation. Both would be of the same length of 74 kilometres, with a breadth of 22 metres at the bottom, and 44 on the surface. Four locks will be needed, each 18 metres wide at the entrance, and with a total length of 180 metres.—On the barometric curves recorded during the third scientific expedition of the *Hirondelle*, by Prince Albert of Monaco. These barometric readings seem to show that the motions of the ship are insufficient to explain the oscillations recorded during the course of a storm, and that these oscillations accompany certain meteorological disturbances without at all aiding to forecast the weather.—On the measuremen t the absolute intensity

of weight, by M. G. Defforges. The apparatus constructed by MM. Brunner Brothers on the principles here laid down has already been applied with satisfactory results to the measurement of absolute gravity at Paris, Lyons, Dunkirk, Algiers, Laghwat, and Nice.—On elliptical polarization by transmission through metals, by M. Georges Meslin. The author here studies the modifications which polarized light undergoes in its passage through metal plates thin enough to be transparent. As in metallic reflection, the two polarized vibrations in the plane of incidence and in the perpendicular plane undergo in relation to each other a certain retardation, while the rectilinear polarization becomes elliptical.—On the application of the phenomenon of transversal magnetization to the study of the coefficient of magnetization of iron, by M. Paul Janet. This question is here studied by means of a method of mutual induction which presents several advantages over other processes, and which may be easily applied to the study of the influence of the medium in the phenomena of induction.—On the decreasing solubility of the sulphates, by M. A. Etard. The author has already shown that between 103° and 190° C. the sulphate of copper becomes less soluble according as the temperature increases. He now finds that most of these salts undergo a certain disturbance at some point of the line of complete solubility, beyond which point the solubility increases less rapidly and even remains almost stationary. Details are given for the sulphates of zinc, manganese, and potassium.—Symmetric disposition of the centres of the four chief continents, by M. Alexis de Tillo. By graphic processes the author finds that the co-ordinates of the orographic centres of the continents are as under: Asia (with Europe) 43° N., 85° E. of Greenwich; Africa, 4° N., 27° E.; North America, 45° N., 102° W.; South America, 14° S., 56° W. The geometric centre of the Old and New World lies in the region of the Azores and Canaries, and the meridian of Delisle (20° W. of Paris) may in some respects be regarded as the *natural* meridian of the globe.

BERLIN.

Physical Society, December 23, 1887.—Prof. du Bois Reymond, President, in the chair.—Prof. Schwalbe gave a detailed account of the research which Dr. Aubel and Prof. Spring have carried out on the rapidity of the interaction between acids and zinc which is mixed with lead.—Prof. Vogel made a statement of his observations of the solar eclipse of August 19. As is well known, the observations during the whole of the lengthy period of totality were unproductive of results at all stations except those in Siberia—which were not much utilized by observers—owing to unfavourable weather. The speaker appears to have been among the most fortunate at Jurjewetz, where he was stationed in company with the Belgian astronomer Niesten, and the Russian astronomers Kortazzi and Belopolski, for at this place the sun was momentarily visible through the clouds. As a matter of fact, several photographs were successfully obtained, on which, as shown by a specimen exhibited, a corona and several protuberances were visible. These photographs, however, scarcely suffice as a basis for any scientific research. Prof. Vogel had also received a photograph of the eclipse taken in clear weather by an amateur in the Ural Mountains; it showed a complete but small corona, and near it is the image of a star, probably Mercury. Unfortunately no details are given about this photograph. The photographer Karelin has secured some very interesting results at Jurjewetz. This observer, using a very sensitive apparatus, had obtained some very successful photographs of the lunar eclipse, which had taken place about a fortnight before the solar eclipse. The plates were only exposed for 1/60 of a second, and working upon this experience he obtained photographs during the solar eclipse by a similar exposure of 1/60 of a second. The results were quite satisfactory, and from this the important conclusion may be arrived at that exposures of the above very short duration may be used during future solar eclipses. Herr Karelin has further taken a photographic landscape during the eclipse, and from a comparison of the time necessary to obtain this with the time required by the speaker's son to obtain a similar picture during full moon, the speaker concluded that the brightness during the solar eclipse was fifty-six times as great as that of the full moon. Prof. Vogel had intended to photograph the spectrum of the corona, but was not successful in his attempt. He further exhibited a photograph of the spectrum of pure oxygen contained in a Geissler tube and made luminous by the sparks from a battery. The photograph was then photographically enlarged, so that it could readily be seen by a large audience at the same time, and in this form it showed the red and green line, together with a long series of bands and lines extending far into the ultra-violet region. Many of the lines described by Dr. Schuster as single could be seen to be double in this photograph. One of the chief things shown by the enlarged photograph is that the oxygen-spectrum of the positive pole, and of the negative pole, as well as the spark-spectrum of the oxygen itself are here combined into a single spectrum. The speaker intends to apply this method of magnifying the photographs to the spectra of other gases, and thus make the enlarged spectra accessible for teaching purposes in the form of diagrams.

January 6.—Prof. du Bois Reymond, President, in the chair.—Prof. Oettingen, of Dorpat, spoke on the explosion of a mixture of hydrogen and oxygen obtained by electrolysis. As is well known, Bunsen has advanced the following view, based on his experiments, on the explosion of electrolytic gas: by the explosive union of the oxygen and hydrogen, when the spark is passed, a temperature of 3000° C. is produced, the water formed being at once dissociated at this temperature; the temperature of the mixture of gases formed by the dissociation then falls, whereupon a new union between the two takes place, and so on; hence the explosion of electrolytic gas is to be regarded as made up of a series of partial explosions following each other in rapid succession. The speaker had intended several years ago to subject Bunsen's theory to an experimental investigation, and hoped to be able to analyze the phenomenon by the use of a rapidly revolving mirror. As a matter of fact, when the mirror was rotated at a suitable speed, the image observed was not that of a single narrow strip of light, but was rather of considerable width; it was not found possible to interpret this image, notwithstanding that the somewhat complicated experiments were repeated many times. An endeavour was next made, with the assistance of a photographer, to obtain a record of the image, which was equally unsuccessful. He then underwent a course of photographic study; and when he had acquired sufficient experience, he last year repeated his former experiments, with a positive result, using the new methods of sensitizing the plates for the less refractive parts of the spectrum, and the most sensitive possible dry plates. The speaker had further shown, by a spectroscopic examination of the light emittted during the explosion of electrolytic gas, that the light is due, not to the combustion of the gases, but of sodium, which is doubtless accounted for by the incandescence of small particles of glass torn off by the passage of the sparks. He hence introduced, in accordance with the method of Dewar and Liveing, portions of finely powdered salts of various metals, such as copper, zinc, lithium, and cadmium, &c., into the eudiometer in which the explosion of the electrolytic gas was to be made, and now obtained, not only excellent spectra of the respective metals, but also quite distinct photographs of the images in the rotating mirror. A plane mirror was used, placed at fixed distances from the eudiometer and camera, which projected the images of the successive events taking place during the explosion on to the flat sensitized plate. The speaker exhibited a series of the photographs thus obtained: these presented the following appearances, most clearly when the salt used was chloride of copper. In the first place, a bright point, corresponding to the place of passage of the spark, from which a short bright ray passed both upwards and downwards in the tube; then secondly, at a fixed distance from this and occupying the whole length of the eudiometer, a bright image intersected lengthways from end to end by zigzag lines and transversely by parallel sinuous waves. The speaker interpreted the above images by referring the intersecting zigzag lines to a series of waves of impulse caused by successive explosions; he considered on the other hand that the sinuous waves are due to the small particles of the metal which are set in motion by the impulse waves, and hopes to render this explanation still more probable by a new series of experiments on the explosion of carbon-disulphide. According to Prof. Oettingen, the experiments of Berthelot, and Vieille, and of Mallard and Lechatelier, have no bearing upon the explosion which he has studied, occurring as it does in a few thousandths of a second, but refer to the combustion which occurs subsequently to the explosion.—Dr. Kötter spoke on the problem of determining the pressure exerted by the earth, discussed the difficulties in the way of estimating the pressure which the earth exerts upon a wall built into it, and stated the limits within

which some theoretical calculations may be relied upon.—Prof. Schwalbe announced that he is engaged in drawing up a Greek nomenclature in connection with physics, and invited the members of the Society to communicate to him any expressions borrowed from Greek which are either rare or difficult to understand.

Meteorological Society, January 3. — The President, Prof. von. Bezold, opened the meeting with a short speech in memory of the late member of the Society, Prof. Kirchhoff, whose many-sided works had not been without importance to the science of meteorology.—The Secretary then made his report on the activity of the Society during the past year, and on the establishment of new meteorological stations in connection with the circle of such stations surrounding Berlin promoted by the Society.—At the election of officers which then followed Dr. Vettin was chosen as President, and Prof. Von Bezold as Vice-President.—Dr. Hellmann spoke on the meteorology of the Iberian Peninsula. During a prolonged stay in Spain in the years 1875-76, the speaker was unable to study the rainfall of the country owing to insufficient data. Since then, however, some 760 annual statements have been published from 70 stations, so that he was now in a position to work out the rainfall, and he presented the results of this in the form of a chart, which formed the basis of his communication. The local distribution of rainfall is very varying. In the district of the Ebro and the whole of the south-east part of the country as far as Carthagena and Old Castile, the rainfall is very slight, the annual fall being about 270 mm.; on the other hand, on the west coast, and in the district of the Pyrenees, the rainfall is considerable, presenting a fall of some 1600 mm. per annum. The maximum fall is found in Serra da Estrella, where it amounts to 3500 mm. The course of the lines of equal rainfall of 300, 400, 600, 800, 1000, and 1600 mm. per annum is extremely curious, and was carefully discussed by the speaker. Two sections through the peninsula, on which the rainfall was represented by ordinates, showed how steep the gradients are when passing from the west coast towards the interior. The speaker threw a good deal of interesting light on the close connection which exists between the agricultural and social conditions of the inhabitants and the rainfall. It appeared that very profound differences have developed themselves between the districts where the rainfall is great and small, and in the latter where the district is well supplied with water or not, these differences completely governing the character and mode of life of the inhabitants. All the stations in common showed a minimal rainfall in the summer, occurring in the months of July and August. In the most southerly stations this minimum falls to 4 mm. for the above two months, whereas in the north-west it rises to more than 100 mm. The curve of maximal rainfall shows three typical forms and three transitional forms. One set of stations shows a maximum in winter, another set has its maximum in the spring, and the third shows it in the autumn, and between these three a graduated transition is observed. The quotient $\frac{\text{maximum}}{\text{minimum}}$ increases rapidly on going south. The difference in the amount of rainfall per annum could only be calculated for thirty-two stations, since it must be based on the records of ten consecutive years at least. The ratio of the extreme to the mean annual rainfall in the north-west, as well as in Central Europe, was two, while in the interior of the country this ratio rose to five. The rainstorms are rarely continuous; they occur chiefly in the morning, and are followed by sunshine: three days of continuous rain, or even of clouds, scarcely ever occur in the whole of Spain. This statement was confirmed by the records of the autographic sunshine recorder. Snow rarely falls in the Iberian Peninsula; the maximum fall of twenty-two snowy days was observed at a station on the upper Douro. At the southern stations snow falls once in thirty years, and it never falls at all at many stations. It is impossible to give any account here of the large mass of further details which the speaker brought before the meeting; they will shortly be published by him in a very extended form.

STOCKHOLM.

Royal Academy of Sciences, January 11.—An account of a memoir by Prof. Ewart, of Edinburgh, on *rigor mortis* and its relation to the putrefaction of fish, by Prof. Smith.—A report of the work done by the Swedish Ornithological Society, by the same.—On the organs and modes of attachment of the marine Algæ, by Count H. Strömfelt.—Mycological studies in Jemtland, by Dr. E. Henning.—On freshwater Algæ from Spain, by Miss M. Lewin.—Astrophotometric studies, by Dr. Charlier.—On the conductibility of illuminated air, by Dr. S. Arrhenius.—Remarks on the paper of Prof. Hoppe, "Zur magnetelectrischen induction," by Dr. Mebius.—On electric currents caused by mechanical pressure, by M. P. A. Siljeström.—Some derivates of naphthostyrite, by Dr. Ekstrand.—On barysite, a silicate of lead from the mines of Harstig, by Messrs. Sjögren and Lundström. On the recent remarks of M. Lebesconte concerning the Cruziana, by Prof. Nathorst.—Demonstration of some propositions of the theory of the elliptic functions, by Dr. Falk.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Institute of Actuaries' Text-book; Part 2, Life Contingencies; G. King (Layton).—Geography for Schools; Part 1, Practical Geography: A. Hughes (Clarendon Press).—Histoire des Sciences Mathématiques et Physiques, tome xii.: M. Marie (Gauthier-Villars, Paris). — The Elements of Graphical Arithmetic and Graphical Statics: J. Y. Gray and G. Lowson (Collins).—Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs; Erster Band, Protozoa, 35 to 41 Lieferung (Williams and Norgate).—Dynamics and Hydrostatics: R. H. Pinkerton (Blackie).—The Farmers' Friends and Foes: T. Wood (Sonnenschein).—Annuaire de L'Observatoire Royal de Bruxelles, 1888, 55 Année (Bruxelles).—Prodromus of the Zoology of Victoria, Decade xv.: F. McCoy (Trübner).—Pflanzenleben. i. Band: Kerner von Marilann (Leipzig).—Le Climat de la Belgique: A. Lancaster (Bruxelles).—Tableaux Résumés des Observations Météorologiques faites à Bruxelles (Bruxelles).—Untersuchungen über die Schneegrenze im Gebiete des Mittleren Innthales: F. R. Kerner von Marilann (Wien).—Journal of the Chemical Society, January, and Supplementary No. (Gurney and Jackson).—Transactions of the Seismological Society of Japan, vol. xi. (Yokohama).—Journal of the Society of Telegraph-Engineers and Electricians, vol. xvi. No. 68 (Spon).—Botanische Jahrbucher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Neunter Band, iii. Heft: Dr. A. Engler (Williams and Norgate).—Actes de la Société Helvétique des Sciences Naturelles, Locle 85 (Neuchâtel).

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